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by ARDON LYON

SUMMARY

In this article I try to give an account of the meaning of phrases of the form 'A causes B' as they are most *usefully* used in everyday life and the applied sciences. This account covers narrower uses of such phrases, but we find that in our usage of the term, 'A causes B' neither entails nor is entailed by 'A is always followed by B'. Logically necessary and sufficient conditions of this general term can be given, however, by reference to *particular* cases of causation. The analysis of these is shown to be irreducibly counterfactual, i.e. such phrases are not definable in terms of statements describing those observable occurrences which constitute the relevant verificationary processes. Some objections to the analysis are considered, and it is tested against various examples.

GENERAL INTRODUCTION

We do not expect philosophers, *qua* philosophers, to discover what are the causes of various occurrences in the world. That job is left to scientists, historians, detectives, doctors, and other practical men. But in the course of their practice, if puzzled whether they really have found out the cause of something, these practical men are sometimes led to ask just what it is for something to cause something else. It is here that the philosopher may be called in. For in difficult cases, and often enough even in easy cases, it is sometimes felt that the question of what it is for something to be the cause of another thing is extremely obscure. If we could clear up this obscurity, we might be able to tell more easily, in the difficult cases, whether something really is a cause, and thus we would gain not only theoretical but also practical knowledge. But the main task of the philosophers is to gain theoretical knowledge, which may then become applied; we are to say what causes *are*.

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* Received 6.xii.65

[1] This article is an expanded version of a paper read to the British Society for the Philosophy of Science at Bristol in September 1964. I have greatly benefited from criticisms and suggestions by members of the philosophy seminar of the University of Durham, especially Dr Doreen Bretherton, and also by Professor J. L. Mackie and the editor and referees of this *Journal*.

How are we to find this out? Nearly all modern philosophers would say that this problem was equivalent to finding out the meaning of the term 'cause'. Now two further problems arise. (1) The term may be used in a variety of ways, in 'ordinary language', by scientists, philosophers, etc. (2) There are different ways of giving the meanings of terms: by translation, by definitions, which may themselves be of many different types, by explaining the way the term is used, by explaining the way statements containing the term are properly verified, and perhaps in other ways.

Problem (1) has given rise to much discussion, and I shall merely indicate briefly what I propose to do in this article, with reference to some historical precedent. It is well known that Aristotle distinguished four sorts of cause. Galileo attacked this view; he wrote that 'only that may be *properly called* a cause which is *always* followed by the effect, and which when removed takes away the effect . . .'[1] (my italics).

In another famous passage, Newton was later to speak of the truth of laws

> appearing to us by phenomena, though their causes be not yet discovered. For these are manifest qualities, and their causes only are occult. And the *Aristotelians* gave the name of occult qualities not to manifest qualities, but to such qualities only as they supposed to lie hid in bodies, and to be the unknown causes of manifest effects; such as would be the causes of gravity, and of magnetic and electric attractions, and of fermentations, if we should suppose that these forces or actions arose from qualities unknown to us and incapable of being discovered and made manifest. Such occult qualities put a stop to the improvement of natural philosophy and therefore of late years have been rejected.[2]

What is interesting about this passage from our point of view is that Newton has not here accepted Galileo's recommendation not to call Aristotelian causes 'causes' and to keep this term only for efficient causes; but he does clearly give the reason why we ought to not bother with them, viz. the pragmatic reason that discussion of these 'causes', far from helping the advancement of science, was actually hindering its progress in that it stopped scientists getting on with the useful task of discovering manifest phenomenal laws and 'causal' relations in the Galilean sense. Thus it seems to me that Galileo was putting forward a linguistic recommendation about what sorts of thing ought to be called 'causes', and a recommendation with a purpose. We might describe this by saying that he was narrowing the concept of *cause.*

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[1] Galileo Galilei, *The Assayer*, XIV; in *The Controversy on the Comets of 1618*, translated by Stillman Drake and C. D. O'Malley, University of Pennsylvania Press, 1960, p. 219. Mario Bunge, in *Causality*, Meridian Books, 1959 and 1963, p. 33, quotes this passage, saying that Galileo 'defined the efficient cause as the *necessary and sufficient condition of something*'. Although the quoted phrase might give this impression, in *The Assayer* XII Galileo clearly allows for multiple causation (*The Controversy on the Comets of 1618*, p. 205). For reference to further historical examples for and against multiple causation see Bunge op. cit., chapter V. My account of multiple causation is given on pp. 7-8, 11-12, and 14-16 *infra*. For further accounts of Galileo on causation see, with references, E. A. Burtt, *The Metaphysical Foundations of Modern Physical Science*, Routledge and Kegan Paul, second edition 1932, chapter III (E), and A. C. Crombie, *Augustine to Galileo*, vol. ii, Mercury Books, 1961, pp. 136-8 and *passim* (note Galileo's 'one effect can have only one basic cause').

[2] Newton, *Opticks*, 3rd edn., corrected, London, 1721, p. 377.

What I shall try to do in this paper is give an account of the meaning of 'cause' in a wide sense, and this too has a pragmatic justification. It is perfectly true that people sometimes use the term 'cause' to refer only to (physically) necessary conditions for the occurrence of an event, or only to (physically) sufficient conditions, or in such a way that it is always inaccurate to speak of a plurality of causes. But I do not think this is a particularly helpful way to talk, because we usually want to use the concept of cause in complex practical situations. Sociologists, economists, historians, anthropologists, psychologists, even engineers, are all very often interested in which of a certain group of factors are causally related to a phenomenon or group of phenomena and which are not. It is true that we could speak of such factors as 'causal factors' as opposed to causes, but I do not think such usage is particularly normal, and there does not seem much *point* in keeping the term 'cause' to refer only to, say (physically) sufficient conditions, when usually we do not know what such conditions are and when, anyway, we have a perfectly good expression available for them, namely, '(physically) sufficient conditions' (or 'physically sufficient causes'). As I. M. Copi points out in his *Introduction to Logic*, when an insurance company wants to find the cause of a fire, it is using the term 'cause' to mean neither (physically) necessary nor sufficient[1] condition; the company wants to find out

> the incident or action which, in the presence of those conditions which are ordinarily present, made the difference between the occurrence or nonoccurrence of the event.[2]

Such causes may be either remote or proximate in the causal chain. Copi also argues that 'there is no single definition of "cause" that conforms to *all* the different uses of that word'[3]; I shall try to show that this is a mistake:

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[1] See also Michael Scriven, 'Explanation and Prediction in Evolutionary Theory', *Science*, 28 August, 1959, vol. 130, footnote to p. 482. 'People have sometimes argued that if A *really* is the cause of X, it must *always* be followed by X. This is to confuse causes with sufficient conditions, and practically to abolish them from the applied sciences, since there are almost no absolutely reliable statements of sufficient conditions there. Causes are not necessary conditions either; their logical nature is complex . . .'. I look upon such an equation as unhelpful rather than as a confusion.

[2] I. M. Copi, *Introduction to Logic*, Macmillan, 1953, 2nd edn., 1961, p. 357.

[3] *Ibid.* p. 358.

that a definition can be produced which covers at least all central modern uses of the word.

What we would like is some sort of general account of what is meant when we say that things (of sort) X cause things (of sort) Y. If X causes Y, X may not always be followed by Y, but it must tend to be followed by Y. However, if examples of X co-vary with examples of Y, either universally or statistically, this may be either fortuitous or the result of a common cause or causes. Again, X may be *a* cause of Y even though increase of X is accompanied by decrease of Y; for example smoking may cause cancer, but a future increase of smoking be accompanied by a decrease of cancer because we will have found a preventive. So how can we explain the meaning of 'X causes Y' in terms of co-variation, as sophisticated Galileans and Humeans would try,[1] when (1) a co-variation may be fortuitous or the result of a common cause, and (2) X may cause Y without X and Y co-varying?

Our second problem was to say what sort of account of the meaning of the term 'cause' should be attempted. In looking for the meaning of 'X causes Y' we may be looking for a definition in terms of logically necessary and sufficient conditions, or we may be satisfied with some other account of the meaning. For example, the term 'vixen' is definable as 'female fox', but it is doubtful whether there are any words in our language with which we can give a similar definition of 'fox'. We can *explain* the meaning of 'fox', however, by pointing out the features characteristic of foxes, even though none of these may be logically necessary for being a fox, and we may be unable to say what, if any, variable group of them is logically sufficient.

But how are we to give either sort of account of the meaning of 'X causes Y'? It appears not to be definable in terms of observable co-variation. For example, in England and Wales between the years 1924 and 1937 a high correlation could be found between the numbers of wireless licences issued and the numbers of notified mental defectives.[2] The correlation coefficient is 0·998; this may be a 'nonsense correlation' and it may not. Yule and Kendall point out that such correlations are liable to occur through space as well as time; for example we would probably find that throughout Europe the percentage of Roman Catholics in the population is inversely proportional to average height, there being more Roman Catholics in the latin south. In general, to argue from a concomitancy to a causality is not infallibly conclusive . . . . Is't not possible, and how know we the contrary, but that something, which always *attends* [X], may be the cause of [Y]?'[1] (My italics).

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[1] For example, R. B. Braithwaite, *Scientific Explanation*, Cambridge University Press, 1953, chapters I and IX, and E. Nagel, *The Structure of Science*, Routledge and Kegan Paul, 1961, chapter IV.

[2] See G. U. Yule and M. G. Kendall, *An Introduction to the Theory of Statistics*, 14th edn., Charles Griffin & Co. Ltd. London, 1950, p. 317 (ch. 13.2). I am grateful to Professor John Wisdom for bringing my attention to this example.

Again, statistics show a high correlation between smoking and cancer; but does the former cause the latter or, as Sir Ronald Fisher claimed, is the correlation perhaps causally explicable in terms of some other factor, such as some unknown genetic predisposition?[2] Whether or not smoking does in fact cause cancer, the statements of statistical variation do not *entail* the statements about causation, and hence statistical variation is not all we *mean* by causation.

If smoking does cause cancer, then we need not expect everyone who smokes, or even everyone who smokes more than a certain amount, to get cancer. If it does cause cancer, however, there must be some *particular* cases of smoking which lead to or produce cancer.[3] Perhaps our task would be made easier if we concentrated on describing what it is for some particular event to cause some other particular event.

AN ATTEMPTED ANALYSIS

In general, if X causes Y, then it must be the case that things of sort X tend to be followed by things of sort Y. We can sometimes have things of sort X without things of sort Y and vice versa, but at the very least we must presumably *sometimes* have examples of X followed by examples of Y. And this is not all: when particular cases of X are followed by particular cases of Y, this following must be not fortuitous, but causal. So if we can say what it is for a thing of sort X to cause a thing of sort Y, then we can give an account of the 'different sorts' of causal relation, or what Copi, I think mistakenly, calls the different 'meanings' of the term 'cause', in the following way.

'X causes Y' means either:
'All and only things of sort X cause things of sort Y', or
'All things of sort X cause things of sort Y, but other things cause things of sort Y as well', or

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[1] Joseph Glanvill, *The Vanity of Dogmatizing*, E. C. for Henry Eversden at the Grey-Hound in St Paul's-Church-Yard, London, 1661, chapter XX.

[2] On this matter some remarks of Freud are much to the point, in his paper 'A Reply to Criticisms on the Anxiety-Neurosis', first published in the *Wiener Klinische Rundschau*, 1895. Translated and published in the *Collected Papers* vol. i, Basic Books, Inc. N.Y. pp. 107-27. The relevant parts of this paper are quoted in my Appendix.

[3] We may one day find a 100 per cent causal correlation between certain conditions taken together with smoking and certain forms of cancer. But this is not our immediate problem, which may be put in the form 'Is smoking sometimes causally efficacious in the production of cancer, so that if we don't want cancer it is wise to stop smoking; or is it causally irrelevant?'

'Some things of sort X cause things of sort Y and some do not, while only things of sort X cause things of sort Y', or
'Some things of sort X cause things of sort Y and some do not, and other things cause things of sort Y as well.'

Instead of 'things of sort X' we might write 'examples of X' or 'cases of X', or sometimes just 'Xs'. 'Cases of X' sounds a bit odd until we replace 'X' by some term, when it reads quite well; our fourth example would then be 'Some cases of X cause Y and some do not . . . etc.'. which would be exemplified by 'Some cases of anxiety cause suicide, . . . etc.'.

By making these distinctions we should in one stroke have got rid of the difficulty of saying what, in general, it is for X to cause Y, when this covers the above multifarious relationships. To be accurate, we should then specify what *sort* of causation we are referring to when we say that X causes Y; and this we already sometimes do by slight differences in terminology. We say that starvation is *sometimes* the cause of death, or alternatively that it is *a* cause of (premature) death, since there can be other causes. We say that heat is a *causal factor* in the production of growth, since we can have the cause without the effect, other causes also being required. And we can say that molecular motion causes heat *tout court*.[1] Indeed, instead of talking of different sorts of causal relation it is perhaps better to say there is only one such relation; what varies is merely whether it holds between all or only some of a group of things or events, or whatever, having a certain common property.

Let us now therefore try to say what it is for a particular thing to cause some other particular thing in some particular case. It is not necessary to consider, for our purposes, what sorts of things are properly called causes and effects—whether events, objects, phenomena, etc.[2] We shall need some terminology to describe in general the particular cases we are considering. I shall use a terminology similar to that of J. R. Lucas in his excellent article 'Causation',[3] the difference being that I shall use small letters to indicate that a thing of a particular sort or characteristic is present in some group of occurrences, events, or phenomena. The small letters function as proper names of particular events, as if we were to name different Englishmen 'englishman$_1$', 'englishman$_2$', etc. Our analysis will allow for the fact that 'X causes Y' neither entails nor is entailed by 'X and Y co-vary'; it also has the advantage of immediately explaining what is meant by talk of the causation of unique events, and of encompassing both multiple causation and functional dependence causation.

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[1] These examples are taken from H. W. B. Joseph, *An Introduction to Logic*, 1906, 2nd edn. 1916, pp. 78-79, Oxford at the Clarendon Press. Like Copi, Joseph concludes from this that the term 'cause' has more than one meaning.

[2] See, for example, the symposium of Vendler, Bromberger, and Dray, in *Analytical Philosophy*, ed. R. J. Butler, pp. 1-31, Basil Blackwell, Oxford, 1962.

[3] In Butler, op. cit. 1962, pp. 39 ff.

We refer to a set of events or phenomena, etc., having the property or characteristic A, as events 'a_1, a_2, a_3, . . .', etc. Thus when we ask what is the cause, or what are the causes, of anxiety, we will speak of the effect (anxiety) as A, and particular occurrences of anxiety as occurrences a_1, a_2, a_3, . . . etc. We then use the small italicized letter *a* as a name-variable; that is, we may speak of some occurrence *a*, which will in fact be one of the occurrences a_1, a_2, a_3, . . . a_n, etc., all of which have the property A.

Similarly we will speak of various occurrences b_1, b_2, b_3, . . . etc., having in common the characteristic B, . . . x_1, x_2, x_3, . . . etc., having in common the characteristic X, . . . etc.

What is now required is that we should say what is meant when it is claimed of a particular event *x*, preceded by or contemporaneous with a series of events *a*, *b*, *c*, *d*, . . . etc., that some of the events, say *a*, *c*, *e*, . . . etc., caused or were causally related to the event *x*, while others, say *b*, *d*, *f*, . . . etc., were not. In what follows I shall speak only of causes preceding their effects, although I think that what I say will be applicable to causes which are contemporaneous with their effects, and perhaps even to causes which follow their effects, if there are any, if the tenses of the verbs used are suitably modified. As E. H. Madden puts it:

> If someone who knew nothing about philosophy or science were asked what he meant when he says 'x is the cause of y', whatever 'x' or 'y' may be, he would probably reply that he means x is ***responsible for bringing about*** y, or x ***produces*** y, or x ***makes*** y ***happen***. These definitions are not very helpful, however, since it is doubtful if 'responsible for bringing about', or 'produces', or 'makes happen' is any more understandable in this context than 'causes'.[1]

Now what is being claimed, surely, when it is said of some particular event *x*, that some of the preceding events *b*, *d*, *f*, etc., were not causally related to *x*, is that their occurrence was irrelevant to the occurrence of *x*; in that particular situation *x* would have occurred whether they occurred or not, even though there may be *other* occurrences of *x*s and *b*s etc., where it would have made some difference whether or not the *b* occurred. But, speaking roughly for the moment, an event say *a*, was a cause of, or was a causal factor in the production of, the event *x*, if in that particular situation, if *a* had not occurred, then *x* would not have occurred[2]; even though, of course, there may be other particular occasions where an *a* (i.e. a thing having the property A) was followed by an *x* (i.e. a thing having the property X) and where the *x* would have occurred whether or not the *a* had occurred; in those situations the *a* was not a causal factor in the production of the *x*. For we cannot correctly claim that an event *a* was a cause of an event *x* if it would have made no difference whether or not *a* had occurred. If *a* had not occurred but some other cause of *x* had, i.e. the events preceding *x*, other than *a*, had been different, then naturally *x* may well have occurred; i.e. it would have been caused by something else. And if all the events had been the same except that *a* had not occurred *an x* may well have been about to occur later, due to other causes; but then it will be not the same event say x_n, but a different event, say x_{n+1}; small letters with subscripts being, in our terminology, proper names of particular events. Thus we can try out the following definition of what it is for a particular event *a* to cause or be a causal factor of a particular event *x*. Since further conditions will be added later, we will call this *Condition 1*.

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[1] E. H. Madden, Introduction to the section 'The Meaning of "Cause" and "Law" ', in E. H. Madden (ed.): *The Structure of Scientific Thought*, The Riverside Press, Cambridge, Mass., 1960, p. 201.

[2] Cf. J. S. Mill, *A System of Logic*, Longmans, Green & Co. 6th edn. 1865, Bk. III, ch.V, section 3. 'Thus, if a person eats of a particular dish, and dies in consequence, that is, would not have died if he had not eaten of it, people would be apt to say that eating of that dish was the cause of his death.'

'*a* was a cause of *x*' means 'In a situation where an event *x* was preceded by a set of events *a*, *b*, *c*, *d*, *e*, . . . etc., if all the events *b*, *c*, *d*, *e*, . . . etc other than *a* had occurred and *a* had not occurred, then *x* would not have occurred.'

Similar definitions can be given of '*a* is causing *x*' and '*a* will cause *x*'. In all these cases *an x* might have occurred later if *a* had not occurred, for some other cause of X might turn up. But then it will be a *different* event *x*. It should be noted that our analysis allows for causal links between the physical and the mental; striking a match causes a flame in precisely the same sense that burning one's finger causes a pain in it, or being locked in a prison cell makes one feel miserable.

Our task might therefore appear to be completed. For have we not now explained what is meant by saying that A causes X? Surely we have described the circumstances in which it is correct to use this expression? But many philosophers would be dissatisfied with this account. For they would like a fuller account of the verification procedure for '*a* caused *x*'. Our definition was uncompromisingly counterfactual, about what would have happened if . . .; and if the meaning of a statement is inextricably bound up with its method of verification, then we have to explain how such counterfactuals can be verified. When we turn to this, we find ourselves confronted with various familiar difficulties. Philosophers who want the logically necessary and sufficient conditions for '*a* caused *x*' in terms of a finite number of possible observations will consider these to be *logical* difficulties; for it turns out that no finite number of statements recording observations and possible observations will *entail* (or be entailed by) the statement that *a* caused *x*. If, however, we are satisfied with an account of the meaning of '*a* caused *x*' in terms of whatever finite number of possible observations in any particular case provide proper *evidence* for '*a* caused *x*', then any difficulty which arises about stating which steps must be taken will be considered merely a *practical* difficulty; i.e. we cannot lay down in advance exactly what steps we will have to take in order to have good evidence that *a* caused *x*, and some of these steps might be in practice impossible to carry out.

If we now attempt to give some account of the verification procedures, we shall discover *why* the relevant evidence-statements never provide logically sufficient conditions for the statement that *a* caused *x*. On account of this, positivistically inclined philosophers may claim that the analysis is incomplete: since they hold that the meaning of any counterfactual is itself unclear and in need of further analysis in terms of the directly observable, the fact that entailment has already been provided—but only by a counterfactual—may well not satisfy them. Others, including myself, will find the analysis satisfactory, since, seeing why evidence-statements can never entail causation-statements, they will claim that we now have a fuller comprehension of what is meant in saying that *a* caused *x*. Just as there is, for example, a logical gap between a phenomenalist account of permanent possibilities of sensations and truths about physical objects, so there is a logically unbridgeable gap between statements about what happens if certain steps are taken and statements about causation. In both cases we can give an account of the verification procedures involved, and in this sense an ('incomplete') account of the meanings of the statements being analysed: in neither case do statements about the verification procedures entail statements about the matters being verified. And just as a comprehension of why and how the phenomenalist account breaks down gives us a clearer comprehension of the meaning of statements about physical objects, so, I hope, even a brief account of how and why evidence-statements verifying particular causal statements do not entail such statements will help us to get a clearer comprehension of the meaning of causal statements. In carrying out this programme we shall modify our analysis as we proceed.

VERIFICATION AND NON-CAUSAL FACTORS

What is the verification procedure for the statement that where a certain event x_1 was preceded by a group of events a_1, b_1, c_1, d_1, . . . etc., if a_1 had not occurred then x_1 would not have occurred? If we can verify this we can verify that in that particular case a_1 was a cause of or at least a causal factor in the production of x_1. To verify this we attempt to set up a situation exactly similar with respect to the events of sort B, C, D, . . . etc., but with no event of sort A. If we find that the experimental set-up with b_2, c_2, d_2, . . . etc. and $-a_2$ is not followed by x_2, a thing or event of sort X, then we can conclude that a_1 was causally related with x_1.[1] Some difficulties relating to this verificationary process will now be considered.

1. *The Analysis of Properties*

We have said that the events a_1, a_2, etc., are events having a common property A. To say that a group of things have a common property A is to classify them. Classification can vary. Thus it might be only in respect of some aspect of A that *a* was causally related to *x*, and so when we produce the experimental set-up lacking the property A we may get misleading results. Suppose we are trying to find out whether broken homes cause juvenile delinquency. Some aspect of broken homes might have a very powerful effect on some types of juvenile delinquency even though there is in general no significant statistical correlation between broken homes and juvenile delinquency, some cases of broken homes producing a less than average correlation with juvenile delinquency owing to further factors present such as outside social help. As J. S. Mill writes, we 'should not allow the freedom of our discriminating faculty to be imprisoned by the swathes and bands of ordinary classification'.[2] One of the most famous examples of unfortunate classification is Aristotle's into earthly and heavenly phenomena; here things are as it were discriminated where they should not be, which shows us that, for example, some aspects of broken homes ought perhaps to be classified together with some other phenomena which, owing to our present lack of knowledge, appear quite disparate. The history of science abounds with examples. Thus failure to classify our events into As and Xs in the correctly relevant manner is the first way in which our verificationary procedure can break down.

2. *Universal Correlation does not entail Causation*

Suppose some peaceful Martians were to see various accurate gunmen shooting people, who then died. Whenever the gunmen pointed their guns in the right direction and there was a loud report, people died. When there was no report, because the gunmen did not fire, or the gun misfired, the victims were unhurt. The Martians might wrongly conclude that the reports caused the deaths. As our analysis shows, they would be wrong, since if *everything* else relevant were the same except there were no report the people would still have died. We know this is true by using guns fitted with silencers. Even if silencers had not been invented, *we* would realise that reports do not (usually) cause death, through our knowledge of similar cases and our knowledge of further details of the relevant causal chain. That is, we know that spears, as well as bullets, cause death, and we know about different sorts of death caused by different sorts of internal bodily harm (compare point 1 above on the analysis of properties). But we may be misled in our verificationary procedure if we do not know how to remove A without at the same time removing another factor which is in fact causally related to X—in this case the firing of bullets. Even in this case, however, we need not be misled if we are able to consider analogical cases, such as that of spears, arrows etc.[1]

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[1] I do not wish to emphasise unduly the human agency in performing these experiments. Nature may have performed them for us, but we can improve matters by careful control of the variables. On this matter, and so-called 'ex post facto' experiments see, besides Mill, Ernest Greenwood, ***Experimental Sociology***, King's Crown Press, N.Y., Morningside Heights, 1945. (Columbia U.P.)

[2] Op. cit. III, VII, 1.

3. *Causes and Conditions*

Many philosophers have pointed out that there are nearly always several causal factors present in the production of an effect, one of which we often single out as 'the' cause. Very often, this is the factor which has just been introduced into the causal network before the effect occurs; but the effect would not have taken place without the 'background' or 'standing' conditions being present also. (These too, as allowed for in our analysis, may vary from situation to situation for an effect of a particular type or class.) Mill believed that we have

> philosophically speaking, no right to give the name of cause to one of them, exclusively of the others. What . . . disguises the incorrectness of the expression is . . . that the various conditions were not *events* . . . but *states*, possessing more or less of permanency; and might therefore have preceded the effect by an indefinite length of duration, for want of the event which was required to complete the required concurrence of conditions: while as soon as that event . . . occurs . . . the effect begins immediately to take place.[2]

Any 'standing condition' seems to be related to a cause in the following way. If a standing condition had been absent, as much as if a cause had been absent, the effect would not have taken place. In all such cases we can conceive of a situation where an event of the same sort or class as the standing condition was absent and which, on being introduced, would be said to cause the event in question. For example, when we strike a match, the oxygen in the atmosphere is not said to cause the flame, but to be a

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[1] Cf. Mill, op. cit. III, IX, 3 and 4.

[2] Ibid. III, V, 3.

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condition of its occurrence. If, however, a match is being struck in an oxygen-free atmosphere, the admission of oxygen may be said to cause the flame to appear. (This example should be compared with what Freud has to say about predispositions as causes, reproduced in the appendix to this article.) So far as scientific knowledge is concerned there seem to be only practical as opposed to theoretical grounds for distinguishing between causes and conditions; we might describe this by saying that 'philosophically speaking' they are indistinguishable.

There appears, however, to be one class of standing conditions or states which present some difficulty. If Jones had not contracted Koch's bacillus, he would not now have tuberculosis. This provides a causal explanation of the tuberculosis. If Jones had died in infancy, or had not yesterday jumped out of the way of a bus that would have killed him, he *could* not now have tuberculosis. But jumping out of the way of buses does not cause tuberculosis.[1] It is perhaps all right to say that if Jones had not jumped he *would* not now have tuberculosis, but there is something rather odd about this, and the use of the word 'could' seems to indicate something approaching a logical, rather than a causal impossibility: we may well feel there is something logically improper in attributing anything at all to Jones (i.e. in saying that *any* present tense statements about him are either true or false) if he is dead. I can only suggest tentatively that such exceptions to our analysis are rather like Strawsonian presuppositions,[2] and that 'Jones's present existence' is not a *state* of Jones, and thus a standing condition of, for example, Jones's contraction of tuberculosis; rather we should call the present existence of Jones a *precondition* of Jones's contracting tuberculosis. It is a logical requirement for correctly positing the present occurrence or indeed the non-occurrence of any event at all involving Jones.

If this is correct, then in order to exclude such events from the class of causes, we should add a further condition to our analysis.

Condition ω. 'a is not a precondition of x (i.e. it is not the case that, if a had not occurred, then it would have been logically improper to say without further explanation either that x occurred or that x did not occur).'

No doubt there are some cases where we would be unsure whether or not it would be improper just to say that x would not have occurred, without adding a rider to the effect that if a had not occurred then the whole of the relevant causal nexus would have been destroyed. But this just reflects the fact that there are some events about which we would hesitate whether to call them causes or preconditions of the occurrence of other events. For example, if the July Bomb Plot had not failed, Hitler would have been killed, and (presumably) the war would have been shortened. Thus its failure can be said to have caused the prolongation of the war. Its *existence* may have caused Hitler to take reprisals, but its *failure* could hardly be said to have caused Hitler to give new orders: if it had not failed he could neither have given nor failed to give new orders.

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[1] I owe this point to Fr P. Fitzpatrick and Mr K. Mills.

[2] P. F. Strawson, *Introduction to Logical Theory*, Methuen & Co. Ltd, London, 1952, pp. 175–9.

Since the question whether an event is a cause or a precondition of a later event is hardly likely to arise for scientists, I propose to drop the matter here. Not only am I unsure whether or not there are other grounds for distinguishing between causes and conditions or preconditions, but as we proceed I do not wish to keep on referring back to this complicating factor Condition ω; it should from now on be taken as read, as no doubt it normally is by scientists who are trying to distinguish the causal from the non-causal in a range of possible factors *within* an accepted existential framework. It should perhaps be emphasized that we could not drop Condition ω in attempting a definition of 'cause' in terms of logically necessary and sufficient conditions; if we did drop it we would be committed to calling various things 'causes' which clearly are not causes but preconditions.

4. *Mill's Method of Difference cannot be used to provide absolutely conclusive evidence of a Causal Relationship*

The set of events b, c, d, . . . etc., preceding x has so far been characterised as an open class, so how can we have any idea whether *all* the events of the sort B, C, D, . . . etc., are present in the new experimental situation? If they are not, then it may be absence of one of these which accounts for the failure of the x to appear.

In the first place, there are certain characteristics which are bound to be absent in a repeatable verificationary situation, but which we have found from countless observations to be causally inefficacious. For example, there are bound to be spatial and temporal differences between an experimental set-up and the original causal situation. We usually find, however, that on repeating experiments in situations which differ, so far as we can tell, only in purely spatial and temporal ways, we always get the same results. It is true that if, for example, we perform experiments with compass needles in different parts of the earth, we get different results; but this is because there is some *further* causal factor which varies with position, namely magnetic field.

On the whole then, we often have excellent experimental evidence that when we produce a situation having the same properties with respect to some of B, C, D, E, . . . etc., the fact that it must be different from our original situation with respect to space and time will not, in itself, produce any causal differences. And so we can proceed by a process of Millian elimination to get inductive but not conclusive evidence about what would have been the case in our original situation if some event had been different. Our *practical* inability to find out, in some cases, whether if *a* had not occurred *x* would not have occurred, owing to the difficulty in producing an exactly similar experimental set-up, does not show that our analysis of what it is for something to cause something else was incorrect; it merely expresses the well known fact that very often we are, in practice, unable to find out what did cause a certain event. But also, since the set of events preceding *x* constitutes an open class, we will never be able to verify *completely* that our experimental processes contain no overlooked and causally relevant differences. Any analysis of causation which made it look as though we could have this sort of complete verification must necessarily be incorrect.

AN OBJECTION CONSIDERED REGARDING MULTIPLE CAUSATION

Our analysis has allowed for a plurality of causes in that there may be several causes of an event *x*, and to call any one of them a cause is to say that if it had not occurred, and all other events had been the same, then *x* would not have occurred. In the case of conjunctive multiple causation,[1] where events of type A.B together (sometimes) cause X, and where both A without B, and B without A never do, there must be some cases of an *a* and a *b* being together followed by an *x*, and here if either had failed to occur, *x* would have failed to occur. About this we can either say, in accordance with the schema on page 5 above, that A.B (sometimes) causes X, but neither A nor B separately do, or say just that A (or B) is a cause of X; for according to our definition of 'A causes X' A is not necessarily by itself a sufficient cause of X, although for some *examples* of A and X it may be.

It has often been suggested, however, that there may be cases where two particular events *a* and *b* are both causes of an event *x*, although if either of them had failed to occur, x would still have occurred.[1] First, consider a case where two men strike a piano key with a force sufficient for either of them alone to have sounded a note—other causal conditions being present such as the hammer being properly connected, the piano not being in a vacuum, etc. Then it seems that if either had not struck, the note would still have sounded, even though the former caused the latter: and so our analysis fails. Now it might be replied that if one had not struck then either the note would have been of a different intensity, and thus have been a different event, or else his striking was not really causally efficacious at all: it merely appears to be so because we have failed to subdivide events accurately enough. However, there may be in nature certain 'indivisible' events for which our analysis fails, in which case we shall have to add an alternative condition to our analysis.

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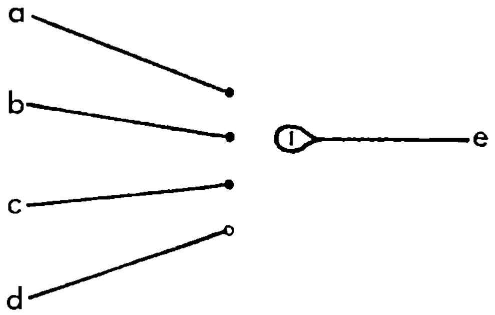
[1] Mario Bunge, op. cit. pp. 119-25, distinguishes between conjunctive and disjunctive multiple causation, claiming that only the latter is genuine *multiple* causation. Disjunctive multiple causation is of course covered by our analysis in a perfectly straightforward way.

As an example of this we may consider the 'all-or-nothing' firing behaviour which takes place across neuron synapses. It may be that if a and b fire simultaneously, or if a alone or b alone fire, then e fires (see diagram). If we really ought to say in the joint case that a and b are both causes of e's firing, rather than that one of them is but we cannot tell which, then we should add a further condition to our analysis of 'a caused x'.



Neurons a, b, and c are exciting, d inhibitory.

Condition 2. 'In a situation where an event x was preceded by a set of events, a, b, c, d, e, . . . etc., then there is at least one event b such that, with all other events remaining as before:

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[1] See, for example, J. L. Mackie's excellent article 'Causes and Conditions', section 2, *The American Philosophical Quarterly*, October 1965, pp. 4-8. The present paper was written before Professor Mackie's article was published, but I had the good fortune to hear him read part of an earlier draft. The examples discussed in my paper were suggested to me by Dr Stuart Anstis and Dr Doreen Bretherton. For the second example see James T. Culbertson, 'Nerve Net Theory' in Harold Borko (ed.), *Computer Applications in the Behavioural Sciences*, Prentice-Hall International, London, 1962.

both (i) if neither *a* nor *b* had occurred, *x* would not have occurred,
and (ii) if *b* had occurred and *a* not, then *x* would have occurred,
but also (iii) if *a* had occurred and *b* not, then *x* would have occurred.

If correct, this Condition 2 will provide (when conjoined with Condition ω) an alternative sufficient condition for '*a* caused, or was a cause of, *x*'. It is not, of course, a necessary condition, because Condition 1 (when conjoined with Condition ω) provides an alternative sufficient condition. The necessary condition of '*a* caused *x*' is given by the conjunction of ω with the disjunction of Conditions 1 and 2. That is,

'*a* caused *x*' = Condition ω & (Condition 1 v. Condition 2) Df.

SOME FINAL REMARKS

J. S. Mill has often been criticised for writing as though his methods of experimental enquiry would produce an 'absolute proof' of a causal connection, particularly by using the Method of Difference. His use of such phrases as 'rigorous certainty' and 'infallible method',[1] his claim that 'Induction is proof'[2] and that such arguments 'are conclusive',[3] back up this criticism. But we need not take such enthusiastic phrases too seriously. Inductive arguments, Mill says, 'may in some remarkable cases amount practically to proof. No such assurance, however, can be had, on any of the ordinary subjects of scientific enquiry.'[4] And when he comes to discuss actual examples of the Method of Difference, he clearly shows why our inferences can never be deductively conclusive.[5] It should be clear, incidentally, that his Methods of Residues and of Concomitant Variations provide evidence of a causal connection, but could not possibly be used in a *definition* of 'cause', since it is always logically possible to have, for example, X and Y co-varying without X causing Y, and vice versa.

I have pointed out that we can never completely verify the causal statements because we can never have logically complete grounds for believing that our experimental situation is exactly the same as the original situation except that no event of sort A is present. Another reason is one which unfortunately—or perhaps fortunately—we have not the space to consider here, namely, that we will not be able to produce absence of A without altering the situation in some way; for unless we do, A *will* occur. (Compare point 2, on pp. 10-11 above.) If we produce absence of A by presence or absence of some factor, say L, then we will have to test whether L is causally related with X, either directly or indirectly through some other factor or factors M, N, O, . . . etc. And once more this is a process which is in theory *always* open to further tests. We should have learned this from Popper if not from Hume. In practice we have difficulty in testing these factors when there are very many variables at work. This is perhaps the main reason why psychology, sociology, and history are not fully 'scientific', and it should be clear that so far as this factor is concerned the difference is one only of degree.

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[1] Mill, op. cit. III, VIII, 5.

[2] Ibid. III, II, 5.

[3] Ibid. III, IX, 6.

[4] Ibid. III, III, 2.

[5] Ibid. III, VII, 1, on analysis of properties; III, VIII, 3 and 4, where we are told we can 'never' have the assurance that two sets of antecedent conditions differ only in one respect; also III, IX, 3, 'we can never have that complete certainty'. Causation is not (merely) '*invariable*' but '*unconditional* antecedence', III, VII, 4. (I should perhaps mention that I would not myself claim that we can be more certain of necessary truths than of contingent ones.)

Finally, a word about 'causal necessity'. Empiricists would like to define 'A causes B' in terms of the fact that Bs do, as a matter of contingent fact, follow As. Without even evaluating the crude mechanistic theory that all our ideas are copies of impressions, which has sometimes been proffered in support of this analysis, we can immediately see that no such definition can suffice. The proposed definition 'All As are (followed by) Bs' is too weak, in that all As may be Bs merely by chance (or because of a common cause or causes), and yet too strong, in that A may cause B without there being even co-variation, let alone universal correlation (see p. 4 above). To get an adequate definition we have to take account of the fact that if A causes B then some As are not merely followed by Bs, but they 'make the Bs occur'. It is for this reason that our definition is irreducibly counterfactual, and it is this that constitutes 'causal necessity', and not some mysteriously intuited necessary relationship. It is therefore only to be expected that no statements about observable occurrences entail causal statements, although the former provide the only conceivable means of verifying the latter. It is well known that Hume defined a 'cause' in his *Enquiry Concerning Human Understanding*, VII, 60, as

> an object, followed by another, and where all the objects similar to the first are followed by objects similar to the second. Or in other words *where, if the first object has not been, the second had never existed.*

As the editor, Selby-Bigge, pointed out in his introduction of 1893, §11, 'the words in italics can hardly be regarded as a paraphrase or equivalent of the main definition'. This matter is admirably expanded by Professor Flew in his book *Hume's Philosophy of Belief*.[1] The observational, experimental steps discussed above, which *test* the truth of '*a* caused *x*' do not exhaust the meaning of the causal statements, which can only be given by a counterfactual. We test it as rigorously as we can, not of course to try to prove it false, but, in Flew's terminology, to 'subject it to strains' which alone can distinguish between causal necessity and mere conjunction. A similar account can be given of the necessity of laws of nature; what makes those with positivistic theories of meaning equate natural laws with constant conjunctions is that anything which falsifies the latter at least tends to falsify the former, although in fact we are often willing to bring in supplementary explanations to keep true those proposed statements of law which we find useful.

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[1] Antony Flew, *Hume's Philosophy of Belief*, ch. VI, 'The Idea of Necessary Connection', especially pp. 129-39. Routledge & Kegan Paul, London, 1961.

CONCLUSION

I have given an attempted definition of '*a* caused *x*' in terms of logically necessary and sufficient conditions, from which we can easily give logically necessary and sufficient conditions for 'A causes X'. I have given some brief pointers towards the verification procedures to be used. That these are fallible is only to be expected, and is no more a criticism of them *qua* verification procedures than is the fallibility of Mill's Methods of experimental enquiry. Much criticism of these latter would only be relevant if they were meant to be definitions of 'cause', which clearly they are not. In philosophical analyses the verification procedures referred to are almost always on a different logical level from the things being verified, and hence do not exhaust the meaning of the term being analysed; the meaning would only be exhausted by giving logically necessary and sufficient conditions. Although statements about relevant verification procedures do not exhaust the meaning of causal statements, since they neither entail nor are entailed by them, we can say that they *explain* the meaning of causal statements, for they give an account of how we do and must set about finding out, always dubitably, whether they are true. If my counterfactual definition is substantially correct, then the 'verification gap' is logically irremediable, and it would be as well for philosophers to give up searching for better definitions of causation in terms of verification procedures and to concentrate on describing different sorts of causation. For myself, I know of no better account than that of Freud, to be found in the following appendix.

Appendix

I append the relevant quotation from Freud. Freud had argued, in an earlier paper, that anxiety-neuroses were caused by sexual noxiae. Dr L. Löwenfeld had attacked this view, stating that very often 'anxiety states appear immediately after a mental shock', and hence it is at least very unlikely that, in many cases anyway, sexual noxiae are causes of the neurosis. In the course of his 'Reply to Criticisms', Freud argues as follows:

> I think we can effect a presentation of the probably very complicated aetiological conditions which exist in the pathology of the neuroses, if we establish the following aetiological concepts:
> (*a*) *Predisposition*, (*b*) *Specific Cause*, (*c*) *Contributory Cause* . . . (*d*) *Exciting* or *Releasing Cause*.
>
> In order to satisfy all possibilities let us assume that we are dealing with aetiological factors capable of quantitative alterations, and consequently of increase or decrease.
>
> . . . we may designate as exciting or releasing cause that which last makes its appearance in the formula, so that it immediately precedes the manifestation of the effect. It is this temporal element alone which constitutes the essence of an incitement. . . .
>
> The factors which are to be described as *predisposition* are those in whose absence the effect would never come about; but which, however, are incapable of alone bringing about the effect, no matter to what degree they may be present. For the specific cause is lacking.
>
> The *specific cause* is one which is never absent when the effect actually takes place, and which also suffices, in the required quantity or intensity, to bring about the effect, provided that the predisposition is present as well.
>
> As *contributory causes* we may comprehend such factors as are not necessarily present every time nor able in any degree to produce the effect alone, but which co-operate with the predisposition and the specific aetiological cause to make up the aetiological formula.
>
> . . . Among the 'indispensable causes' we find several which are also present in the aetiological formula of many other conditions beside anxiety-neurosis, thus showing that they have no particular relation to individual disorders; *one* of these causes, however, stands out with special prominence, in that it is found in no other or in very few aetiological formulae, and this has the claim to be called the *specific* cause of the disease in question. Further, predisposing factors and specific causes are particularly clearly distinguished in cases where the former have the quality of long duration and little alteration in their condition, whereas the specific cause corresponds to a factor which has recently come into action.
>
> I will attempt to give an example of this complete aetiological scheme:
>
> *Effect*: Phthisis pulmonum. (Pulmonary tuberculosis)
> *Predisposition:* For the most part an hereditary disposition of the organs concerned.
> *Specific Cause:* Koch's bacillus.
> *Contributory Causes:* Everything that lowers resistance; emotion as well as infections or colds.
>
> The scheme for the aetiology of anxiety-neurosis seems to me to run similarly, thus:

> *Predisposition:* Heredity.
> *Specific Cause:* A sexual factor in the sense of a deflection of sexual tension from the psychical field.
> *Contributory Causes:* All 'ordinary' injurious factors: emotion, fright, as well as physical exhaustion through illness or over-exertion.
>
> . . . heredity alone is not usually able to produce an anxiety-neurosis, but waits for the incidence of a sufficient quantity of the specific sexual noxia. The existence of an hereditary factor does not spare us therefore the search for a specific factor, upon which, incidentally, all therapeutic interest also depends. For where are we to begin therapeutically with an hereditary aetiology?

It is this last sentence which is relevant to Sir Ronald Fisher's views on smoking and cancer.

I hope that the causes which Freud here describes would all be covered by my definition; I had this example in mind in producing it.

The City University
London

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Popper versus Kirk: A Controversy in the Interpretation of Greek Science*

by G. E. R. LLOYD

It is a minor scandal that specialists in the interpretation of early Greek thought have, in general, paid so little attention to Professor Sir Karl Popper's provocative foray into their territory in his Presidential Address to the Aristotelian Society (Popper, 1958). That paper was answered by Professor G. S. Kirk in an article in *Mind* (Kirk, 1960), and since then both scholars have contributed a second paper which develops parts of their respective views (Kirk, 1961, Popper, 1963). But subsequently the debate has fizzled out. Nor, so far as I know, has any other classical scholar besides Kirk taken part (though Mr J. E. Raven repeated some of Kirk's criticisms of Popper in a paper read to the joint conference of the Hellenic and Roman Societies in 1965). And yet their controversy raises two fundamental issues, first the major philosophical question of the logic of scientific discovery, and secondly the historical question concerning the characteristics and methods of Greek natural philosophy. It is this latter question that I wish to take up here.[1] In particular I wish to broaden the field of the discussion to take the whole of early Greek speculative thought (and not merely the work of the philosophers) into account. And I shall suggest where certain generalisations which may apply to those writers whose interests were primarily cosmological should be modified when we take all the available evidence for the period down to Plato into account. I shall, however, begin by rehearsing briefly the main arguments that have been advanced by Popper and by Kirk concerning the Presocratic philosophers.

Popper's original paper was entitled 'Back to the Presocratics'. 'What I want to return to', he said, 'is the simple straightforward *rationality* of the Presocratics. The simplicity and boldness of their questions is part of it,

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* Received 18.ii.66

[1] An earlier version of this paper was read to the History and Philosophy of Science Seminar at Cambridge and to Professor Sir Karl Popper's Seminar at the London School of Economics. I am most grateful to those who have criticised my paper on those occasions and in correspondence and especially to Sir Karl Popper and Professor Kirk. The defects that remain are, of course, entirely my own responsibility, but I am conscious that they would have been far greater but for the patient and courteous criticisms I have received.

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but more important still is the critical attitude which, as I shall try to show, was first developed in the Ionian School' (Popper, 1958, p. 1). But Popper's purpose was not merely to praise Thales, Anaximander, Anaximenes and the rest, but also to draw lessons from their work for the history of science as a whole. In particular he used the evidence relating to the work of the Presocratics in order to attack, as he had done on previous occasions, the 'Baconian myth according to which science starts from observation and then slowly and cautiously proceeds to theories' (Popper, 1958, p. 3). 'Western science', as he put it, 'did not start with collecting observations of oranges, but with bold theories about the world' (Popper, 1958, p. 3). He argued that most of the ideas of the early Presocratics, and the best of them, 'have nothing to do with observation', and to support this claim he referred to such examples as Thales's notion that the earth floats on water, and Anaximander's idea of the earth's free suspension in space. He dealt in some detail with other Presocratics too, particularly with Heraclitus, where he argued strenuously against the view taken by Kirk (1957) according to which Heraclitus himself held not the strong thesis that every single thing is constantly and invariably in change, but merely the much weaker thesis that the world as a whole undergoes continuous change.

Popper presented his theses in what can only be considered a provocative manner, saying that 'the exciting story of the development of the problem of change' (for example) appeared to be 'in danger of being completely buried under the mounting heap of the minutiae of textual criticism' (Popper, 1958, pp. 9-10). 'In this paper', he put it, 'I speak as an amateur, as a lover of the beautiful story of the Presocratics. I am not a specialist, nor an expert: I am completely out of my depth when an expert begins to argue what words or phrases Heraclitus might have used, and what words or phrases he could not possibly have used. But when the experts replace a beautiful story, based on the oldest texts we possess, by one which—to me at any rate—makes no sense any longer, then I feel that even an amateur may stand up and defend an old tradition' (Popper, 1958, p. 2). When Kirk (1960) came to reply, he defended the practice of minute textual criticism against Popper's somewhat indiscriminate attacks.[1] And he put forward a different interpretation of certain specific Presocratic doctrines from that which Popper had advanced. In particular he corrected Popper's suggestion that in Anaximander winds were responsible not only for the weather, but for 'all other changes within the cosmic edifice' (Kirk, 1960, p. 332).[1] And thirdly where Popper had argued that Presocratic philosophy contained some 'staggering anticipations of modern results', Kirk advanced a more sceptical and seemingly more judicious view of the extent to which such anticipations are to be found.

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[1] Popper (1963, p. 157) has since protested that he neither said nor suggested that consideration of the words or phrases Heraclitus might have used was irrelevant: he merely disclaimed competence in the field of linguistic criticism. Nevertheless some of his remarks can be construed as attacks not on a particular expert nor on particular conclusions reached on the basis of a close study of the text, so much as on the experts, and the practice of textual criticism, in general.

But if on these mainly peripheral issues Kirk made a number of telling points, he was less successful in his criticisms of the central thesis of Popper's paper, the anti-Baconian argument. The first part of Kirk's paper was a critique of Popper's theory of scientific methodology. There he began by considering why, that is for what historical and personal reasons, Popper arrived at his—as Kirk thought—utterly mistaken view.[2] But on the main point at issue Kirk simply asserted that 'science ultimately does start from observation'.

> Naturally, most attempts to extend scientific knowledge start immediately from an idea or intuition, which is then tested 'scientifically' and accordingly approved, modified, or rejected. But the idea or intuition which acted as the starting-point of any such particular process is itself the culmination of a previous process or series of processes which must have been 'inductive' in some valid sense because it must ultimately have been based on an indefinite number of particular observations.... What Popper has done, then, in his description of the process of scientific discovery, is to ignore the essential preliminary stage of making observations, of building up a complex structure of experience out of which, by some kind of inductive process, come intuitions or universal theories' (Kirk, 1960, pp. 321-2).

But Kirk's notion of this 'essential preliminary stage' in the process of scientific discovery seems not only true but a truism[3]: and it may be remarked that it does not help us to discriminate between what is and what is not science.

But apart from the general question of the nature of scientific method, there is a substantial disagreement between Popper and Kirk on the place of observation, and of what Kirk calls common-sense, in Presocratic philosophy. In his Presidential Address Popper took Kirk to task for his remark that 'it cannot be too strongly emphasised that before Parmenides and his apparent proof that the senses were completely fallacious . . . gross departures from common sense must only be accepted when the evidence for them is extremely strong' (Kirk, 1957, p. 197; Popper, 1958, p. 16). In his reply Kirk made a small but perhaps important change in the formulation of his position: 'Naturally many of the conclusions of the early Presocratics were contrary to common sense; but they were not gratuitous departures from it ("gratuitous" expresses my meaning better than "gross") since they appeared to be entailed by arguments which themselves depended on observation and common sense' (Kirk, 1960, pp. 335-6). Kirk devoted a paragraph to this in the 1960 paper, but a whole article (Kirk, 1961) entitled 'Sense and Common-sense in the Development of Greek Philosophy' was published the next year[1] in which he went through not only the Presocratics but the whole of classical Greek philosophy considering which of their theories could be accounted exceptions to his rule that the Greek philosophers did not gratuitously depart from, or overstep the bounds of, common sense. 'Common sense' is at the best of times a dangerously vague and subjective term. In his opening paragraphs Kirk gave various examples to explain the distinction between a notion allowed by common sense and one precluded by it, but the examples he gave suggest that he would be reluctant to concede that these categories may be culture- or society-specific.[2] At least when he draws a general contrast between Greek myths and those of their ancient Near Eastern neighbours he appears to discount the possibility that some of the 'symbolist extravagances' of the Egyptians may have seemed just as commonsensical to *them* as the Greek beliefs about the sun or the sky or the origin of things did to the Greeks.

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[1] When he came to reprint his original paper Popper (1963, pp. 136 ff.) modified some of his statements and introduced an explanatory note to meet Kirk's criticisms at this point.

[2] In his reply Popper (1963, pp. 154 ff.) has protested that Kirk has misrepresented his position, particularly by suggesting that his, Popper's, view of scientific methodology is an intuitionist one.

[3] Compare Popper's recognition of the role of 'experience', including observational knowledge, in the origin of problems (Popper, 1963, p. 155), quoted p. 26, note.

But Kirk's chief concern was with the philosophers. He granted that there are quite a number of their theories that offend common sense, but he suggested that, in general, the philosophers took refuge in such theories only when they believed themselves to be compelled to do so by the demands of logic. He offered, in effect, two types of explanation for the theories in question. First such a theory may be a generalisation arrived at by an over-bold induction from a single observation or set of observations. The Pythagorean doctrine that the world is somehow made out of numbers is the prime example. But this number-physics, as Kirk pointed out, seems to have resulted from an important new observation—that the musical scale is basically numerical. Secondly there are cases where a startlingly improbable theory arose directly as the result of following an initial axiom to its apparently logical conclusions (see, e.g. Kirk, 1961, p. 111). Here Parmenides provides the clearest example. Kirk allowed that from the point of view of common sense Parmenides's doctrine of Being is 'completely improbable and indeed utterly nonsensical'. But, he went on, 'the Eleatic conclusion about Being was the direct result of a logical process of inference—admittedly an incorrect one—and . . . it was logic alone that led Parmenides to overthrow the world of common-sense' (1961, pp. 110-1).[1] He dealt similarly with Empedocles, for example, arguing that certain of his 'rather quaint' cosmological ideas were 'initially imposed upon' him 'by the demands of symmetry in working out his cyclical scheme, itself necessitated by his premises of essential unity and no becoming' (1961, p. 111). As for Anaxagoras, Kirk agreed that his physical theory 'must have seemed inherently improbable, as well as intolerably complex, to the common-sense view of interested amateurs', but went on: 'we must recognise that a degree of improbability was inevitably entailed by the attempt to retain the sense-world at all' (1961, p. 112). The Atomists too he saw as taking a 'momentous step away from the phenomenal world of common-sense'. But again their theory 'depended from a chain of reasoning, one which owed much to that of the Eleatics. It was the apparent cogency of this reasoning', Kirk concluded, 'that gave Democritus the confidence to proceed beyond the senses' (1961, p. 112).

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[1] This paper originated in one read to a classical conference in 1958, that is to say it was based on ideas which Kirk had developed before Popper's first paper (1958) had appeared.

[2] 'Common sense', he said (Kirk, 1961, p. 105), 'might not prevent a man from believing that the sun is a large ball of fire... On the other hand the belief that the sun is made out of triangles, or is rowed across the sky with oars, may be felt to offend common sense, because there are no obvious elements of our experience that can be combined without contradiction to form such a picture.'

Kirk continued with Plato and Aristotle, the Epicureans and the Stoics, but failed there too to find gratuitous departures from common sense. Indeed considering his idea of what would constitute such a gratuitous departure and his explicit statement that 'philosophers normally avoid hypotheses that are gratuitously contrary to common-sense' (1961, p. 105), it would have been quite surprising if he had found many such theories. He did, in fact, cite two possible examples in the history of classical Greek philosophy,[2] Xenophanes's obscure idea that eclipses are caused by the sun 'wandering off into a region of the earth uninhabited by men', and the doctrine of innumerable worlds attributed to Anaximander. But the former he saw as an ironical parody of the 'excessive dogmatism and naive theorisation of the Milesians' (1961, p. 108), and the latter, he pointed out, may well be a doxographic misrepresentation of Anaximander. Yet to show that the early Greek philosophers avoid theories that are purely whimsical does not seem particularly interesting (even if it serves to counteract a certain tendency, on the part of some commentators, to assume that many of their theories are quite arbitrary). And one might draw a rather different conclusion from the one Kirk suggested from the material on which he commented. What might be thought remarkable about that material is not the point that Kirk draws attention to, that there are so few *gratuitous* departures from 'common sense', so much as that there are so *many departures* from 'common sense'—not that when they suggested theories that offend against the evidence of the senses, the Greek philosophers thought they had good reasons for doing so, but rather that they felt themselves constrained to do so so often.

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[1] In his first paper Kirk had stated emphatically that those of the Presocratics' theories that are contrary to common sense are not gratuitous departures from it 'since they appeared to be entailed by arguments which themselves depended on observation and common sense' (1960, pp. 335-6). But it would seem impossible to claim this to be true of Parmenides's doctrine of Being.

[2] Kirk noted that Heraclitus would be another instance if he held the strong thesis that every individual thing is in perpetual flux (though Kirk denies, of course, that this was Heraclitus's thesis) (1961, p. 109).

Subsequently Popper (1963), too, has made a second contribution to the debate, where he argued that Kirk had misrepresented his view of scientific methodology[1] and reaffirmed his general thesis on the Presocratics, and then concentrated most of his attention on two specific points in the interpretation of Heraclitus.

The main issue between Popper and Kirk is a general one in the philosophy of science. At the same time they have expressed divergent views on the role of observation in the early Greek natural philosophers, and this raises a historical issue concerning the methods of early Greek science. It is this issue that I wish to follow up in the rest of this paper. While Popper and Kirk have confined their attention, so far, to figures who appear in the histories of Greek *philosophy*, I wish to extend the area of the discussion to take the evidence for other parts of early Greek science into account. There are interesting and important differences both between the practice of different early Greek scientists, and between the theories some of them entertained on the subject of the methods to be used in the investigation into nature. A study of the medical writers, for example, can, of course, in no way invalidate generalisations that are meant to apply to the cosmologists alone: it may, however, help us to discover where the methods favoured by particular philosophers are typical or atypical of early Greek science taken as a whole.

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[1] Popper (1963, p. 155) briefly restated his position on the question of the starting-point of science as follows: 'I do not say that science starts from intuitions but that *it starts from problems*; that we arrive at a new theory, in the main, by trying to solve problems; that these problems arise in our attempts to understand the world as we know it—the world of our "experience" (where "experience" consists largely of expectations or theories and partly also of observational knowledge—although I happen to believe that there does not exist anything like *pure* observational knowledge, untainted by expectations or theories).'

Apart from the philosophers the other major strand to Greek science in the pre-Platonic period is represented by certain of the Hippocratic writings. It is true that this material is difficult to handle because very few of the texts can be assigned to a specific author and the dates of most of them cannot be determined at all precisely. But against this it must be said first that the *general* period within which almost all of them fall can be fixed reasonably certainly, that is from the mid fifth to the late fourth century B.C., and secondly that they have one enormous advantage (from our point of view) over the remains of the Presocratic philosophers, namely that we possess not just a series of isolated fragments quoted by later writers, but a considerable number of quite extensive treatises. These constitute, in fact, one of our main sources not merely for Greek medical theory and practice (that is obvious), but also for Greek biology in general and even for certain aspects of contemporary Greek physics and cosmology. What is more, they provide some striking early examples of the painstaking collecting and reporting of particular facts. In attempting to assess how far there are any signs of inductivist tendencies in early Greek science taken as a whole we should, then, consider such works as the Hippocratic treatises *On Epidemics*.

Books I and III of the *Epidemics* probably form a single work which appears to date from some time in or around the last decade of the fifth century B.C. (Deichgräber, 1933, p. 16; Bourgey, 1953, p. 32). These books contain first what are known as the 'constitutions', that is general descriptions of the climatic conditions accompanying certain outbreaks, and then a number of detailed case histories in which the daily progress of a particular patient's illness is set out. The entries under each day vary in length from a single remark to a fairly detailed description,[1] and the observations continue until the patient either dies (as happens in the majority of cases) or recovers, in some cases as late as the 120th day from the onset of the disease. These treatises have always been famous for their cool, professional attitude to clinical medicine, and their careful and methodical observations of the symptoms of various conditions. One may also remark the absence of any dogmatic overall theory of the origins of diseases such as we find often enough elsewhere in the Hippocratic Corpus (an extreme example is provided by the work *On Breaths* which calmly states that the cause of all diseases is one and the same, namely 'air', chapters 2, 4 and 5, *Corpus*

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[1] E.g. *Epid.* III, Case 3 (First Series): 'Fourth day. Vomited scanty, bilious, yellow vomits, and after a short interval, verdigris-coloured ones; slight flow of unmixed blood from the left nostril; stools unaltered and urine unaltered; sweat about the head and collar-bones; spleen enlarged; pain in the direction of the thigh; tension, soft underneath, of the right hypochondrium; no sleep at night; slight delirium' (trans. Jones, 1923, p. 223).

Medicorum Graecorum, I, i. 92, 13 ff.). We might ask, then, how far it would be justified to see the case histories of the *Epidemics* as serving the purpose of preliminary collections of observations prior to the proposal of pathological doctrines. How far would it be right to represent this writer as proceeding inductively from particular case histories to general theories?

First of all we must be clear about the writer's motives for undertaking and setting out his case histories in the way he does. Like most of the Hippocratic medical writers, he attaches greater importance to prognosis than to diagnosis. The object of assembling the collections of case histories is not so much to provide information which would enable the causes of diseases to be established, as to provide information which would help doctors to predict the outcome of particular diseases (especially, of course, whether the patient would die or recover). But a further point concerns the way in which the observations are set out, that is the way in which the writer begins with the first day of the disease and makes an entry for each subsequent day on which any significant change was observed until death or recovery. The observations are methodical and even meticulous. But the writer has an additional motive for presenting these observations in the way he does, and indeed for carrying out his *daily* check, *over and above* any laudable desire to be thorough in his work, that is he adheres to the common Greek medical theory that the course of acute diseases is determined according to what are called 'critical days'. Thus in the third Constitution in Book I (ch. 22) we find several such passages as the following: 'Most cases had a crisis on the fifth day from the outset, then intermitted four days, relapsed, had a crisis on the fifth day after the relapse, that is, after thirteen days altogether. Mostly children experienced crises thus, but older people did so too. Some had a crisis on the eleventh day, a relapse on the fourteenth, and a complete crisis on the twentieth. But if rigour came on about the twentieth day the crisis came on the fortieth' (trans. Jones, 1923, pp. 179-81). And later in the same book (ch. 26) the writer produces a detailed table of critical periods for diseases that had crises on even, and for those that had crises on odd, days.

It is clear that these books of the *Epidemics*, for all their appearance of being a set of systematic observations carried out in a spirit of 'disinterested' inquiry, owe something of their overall form to the writer's preconceived idea of the importance of critical days. But to this two points should be added. First one should not underestimate the experiential basis for the notion of 'critical' days. Jones, for example, remarked that it is a commonplace that some diseases tend to reach a crisis on a fixed day from the commencement, although the day is not absolutely fixed, nor is it the same for all diseases, and he went on to suggest a particular connection between the theory of critical days and the periodicity of malaria (Jones, 1923, Intro. pp. liv-lv). And secondly we should note the relation between theory and observation in *Epidemics* I and III. The writer is certainly disposed to believe that 'critical' days follow certain patterns. But the specific rules he suggests are evidently based on his particular observations, that is the case-histories, including, no doubt, many others besides those recorded in our texts. Thus in chapter 21 of the third Constitution, having noted that 'when there was a crisis on the seventh day, with an intermission of six days followed by a relapse, there was a second crisis on the seventh day after the relapse', he mentions an example: 'in the case of Phanocritus, for example, who lay a sick at the house of Gnathon the fuller' (trans. Jones, 1923, p. 179). And in the same chapter he refers by name to two patients whom we may assume to be identical with the subjects of the first and second case-histories in the series that immediately follows (although if the Silenus mentioned is the same man, this example shows that the writer is capable of careless mistakes in using the case-histories, in this instance concerning the day on which the patient died). It is not, then, that the writer conducted his observations simply in order to confirm rules *that he had already formulated in detail.* Rather those detailed rules are, for the most part at least, generalisations which he made on the basis of his particular observations (cf. Deichgräber, 1933, pp. 20-21).[1] And although the writer only rarely makes suggestions relating to the causes of the diseases he was studying, when he does so he is again clearly building on the evidence of the detailed case-histories, as for example in the third Constitution when he remarks on the predominance of the humour blood and several times notes the connection between loss of blood and recovery (chs. 14 and 15, of Deichgräber, 1933, p. 13).

Not all Greek science, even in the period before Plato, is as heavily inclined towards abstract theorising, and as impatient of empirical investigation, as most of the Presocratic philosophers appear to have been. *Epidemics* I and III, in particular, report detailed observations which served as the basis for generalisations concerning the periodicities of diseases. But it is not only their actual practice that suggests that different attitudes were taken by different early Greek scientists on the question of the proper method of conducting the inquiry into nature. We should next consider what the Greek authors themselves have to say on the subject of the aims of science and on the nature of scientific method. It is true that most of the important texts relating to these questions come not from the pre-Platonic period, but from the fourth century. Aristotle is obviously our most important single source if we try to investigate what the Greeks thought they were doing when they were doing science, or what ideas they had on the nature of the method of inquiry to be used in science. And yet it would be wrong to assume that the Aristotelian doctrine of *episteme* is the only, even if it may be the most representative, Greek notion of what 'science' is. Again the anonymous medical writers supply interesting evidence relating to the period from the mid-fifth to the mid fourth century B.C.

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[1] Most of these rules take the form of generalisations which (he claims) apply 'for the most part' or 'in the majority of cases', and he notes instances where the pattern of critical days for similar conditions was dissimilar. Thus in chapter 20 of the third Constitution he remarks that two brothers fell ill at the same time but had their first crises on different days. Cf. also *Prognostic*, chapter 20, the author of which observes that critical periods cannot be calculated in whole days any more than the solar year or the lunar month can.

The author of *On Ancient Medicine*, for example, who was probably writing around the end of the fifth century, shows what, for his time, is a quite surprising grasp of problems of method and of the distinctions between different types of inquiry. In chapter 1, particularly, he distinguishes different inquiries not merely by their subject-matter, but by their method, especially by whether or not they need to employ what he calls *hupotheseis*—postulates or assumptions. Medicine, he claims, has no need of such assumptions, differing in this from 'obscure and problematic subjects, concerning which anyone who attempts to hold forth at all is forced to use a postulate, as for example about things in heaven or things under the earth: for if anyone were to speak and declare the nature of these things, it would not be clear either to the speaker himself or to his audience whether what was said was true or not, since there is no criterion to which one should refer to obtain clear knowledge' (*CMG*, I. i. 36, 15 ff.). The writer distinguishes, then, between those inquiries where some sort of 'postulate' is necessary, and those like medicine (as he believes) where it is not. But when he cites astronomy, meteorology and geology as examples of inquiries where a postulate *is* necessary, it is not that he approves of the use of a postulate in these subjects but not in others. On the contrary, the very fact that they have to make use of a postulate is enough, in the writer's opinion, to condemn those inquiries as worthless—for that surely is the implication of the remark 'it would not be clear either to the speaker himself or to his audience whether what was said was true or not, since there is no criterion to which one should refer to obtain clear knowledge'. This is, in effect, a statement of the need for scientific theories to be testable: speculation about what goes on in the sky and under the earth is worthless because unverifiable, at least according to the author's standards of verifiability. And yet when we turn to the theories concerning the component substances in the body and the origins of diseases that are proposed by the author himself, there is perhaps little need to point out that they are similar in kind to, and almost as arbitrary as, the doctrines that he refuted with such damaging arguments.[1]

Chapter 1 of *Ancient Medicine* provides important evidence of what one writer in the period before Aristotle thought on certain methodological issues. At first sight his demand for 'clarity' (*to saphes*) might seem comparable with the Aristotelian insistence on certain, unshakable knowledge. But the two ideas are quite different. Aristotle speaks of two types of cognitive state as unfailingly true, *nous*, the understanding of the primary self-evident premisses or starting-points of knowledge, and *episteme*, 'scientific' knowledge based on the demonstrative syllogism (see, e.g. *An. Post.* 100 b, 5 ff.). *Ancient Medicine*'s idea of 'clear knowledge' is much less sophisticated, but clarity appears to belong, in his view, only to notions for which there is overwhelming support in the direct evidence of the senses, and he certainly rejects conjecture for the sake of conjecture as worthless.

What is more *Ancient Medicine* also sets out certain ideas on the origins of medicine, and these too are worth comparing with the later, more influential notions of Aristotle on the development of different kinds of knowledge. The views expounded on this subject in *Metaphysics* A chapters 1-2 are well known. There Aristotle distinguishes between 'experience' 'art' and 'science', but he makes no secret of his preference for those branches of knowledge that are purely speculative and unproductive, and he suggests that the highest form of knowledge, wisdom, arises from the sense of wonder (982 b, 11 ff.). *Ancient Medicine* deals, it is true, mainly with the origin of one specific 'art', medicine, but his views on this are striking. He treats medicine as a development of dietetics. He traces its origin to the trial and error experiments that men in their primitive state must (he believes) have carried out on their food in order to make it digestible. 'Trying many other things out on their food,' he says in chapter 3 (38, 16 ff.), 'they boiled and roasted and mixed it . . . adapting it all to the nature of man. . . . But what juster or more fitting name could one give this discovery and inquiry than medicine?' And when he comes in a later chapter (20, 51, 6 ff.) to talk about the inquiry into nature more generally, he says: 'I think that clear knowledge about nature can only be gained from medicine, and from no other source.' Unless one studies medicine, he goes on, one cannot know 'what a man is, by what causes he comes to be and so on exactly'. His view, then, is that the 'arts' (and his term *techne* is one that covers not only technology, but also inquiries that we should classify as scientific rather than technological) develop as a response to practical human needs.[1]

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[1] He criticises medical theorists who had postulated 'the hot' 'the cold' 'the wet' or 'the dry' as the component substances in the body, but in his own admittedly rather more complex doctrine (chapter 14) he mentions as components such things as 'the sweet' and 'the bitter' (see further, Lloyd, 1966, pp. 69-70).

Much more could be said on the theme of the variety of the ideas on the nature, aims and origins of 'the inquiry into nature' that are developed by different Greek writers in the fifth and fourth centuries. But while the basis of my approach has been that the evidence for the cosmologists is only a part (though the best known and in many ways the most important part) of the total material available in a study of early Greek science, it is now time to return to the general problem of the relation between, and the relative importance of, conjecture and observation in early Greek science, bearing in mind the differing views that have been expressed by Popper and Kirk on the Presocratics, Popper maintaining that the best of the Presocratics' ideas have nothing to do with observation, and Kirk on the other hand insisting on the part played by observation (and commonsense) in their theories. In part the disagreement between Popper and Kirk at this point seems to be a disagreement on values or emphasis, rather than on facts, and to reflect the different aspects of Presocratic speculation they have chosen to stress, and indeed to some extent one may, without contradiction, agree with what each of them has suggested. At the same time the evidence of the medical writers introduces certain new factors into the assessment of the balance between the dogmatic and the empirical strains in early Greek science as a whole, and I may now attempt to summarise the general conclusions that I think can be drawn on this issue.

First several of the theses concerning the origins of Greek science that Popper has developed (with some, at least, of which Kirk too is in agreement) must surely be accepted. The first is that Greek science begins with global questions, not with detailed, specific ones, let alone with collections of observations. It begins with such problems as how things began and what they are made of. And of course the question of origins, particularly, had always been a favourite subject of myths, so that the Milesian philosophers' theories of the beginnings of things may be seen as rationalistic answers to a question that had already been posed, and answered, in mythical terms in, for example, Hesiod's *Theogony*.

Secondly the role of rational criticism and debate in the early stages of Greek science is amply demonstrated by an impressive body of evidence. Whether or not Anaximander developed some of his doctrines in reaction to those of Thales, and Anaximenes to those of Anaximander, with later philosophers we have texts in which they refer unmistakably to the theories of their predecessors, often mentioning them by name. Xenophanes evidently satirises Pythagoras (Fr. 7), besides criticising Homer and Hesiod for their anthropomorphism. Heraclitus criticises both Pythagoras and Xenophanes by name (Fr. 40). Whether Parmenides had Heraclitus, among others, in mind in Fr. 6 is disputed, but the remains of Empedocles and of Anaxagoras contain obvious echoes of Parmenides. And here the medical writers provide supplementary evidence which tells a similar story. Besides the many Hippocratic treatises which criticise the theories of other medical writers, *On the Nature of Man* (ch. 1) refers explicitly to the philosopher Melissus,[1] and *On Ancient Medicine* (ch. 20) to Empedocles. Although our information on the dates and places of residence of many writers is imprecise, we have enough evidence to be sure that at the end of the fifth century theorists working in widely separated parts of the Greek world were quite often acquainted with one another's doctrines in some detail. The ease with which ideas seem to have travelled, in a period when there were still formidable barriers to the passage of men and goods, is remarkable and deserves to be more fully investigated.

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[1] Compare the views on the origin of civilisation and the arts reported in Diodorus, I, 8, 1 ff. (which are generally associated with Democritus).

Then quite early in the history of the 'inquiry into nature', indeed not long after the Milesians themselves, there are second-order disputes, debates not between one particular theory or explanation and another, but on such questions as the bases of knowledge in general.[2] This particular problem is, of course, largely the province of the philosophers.[3] But what is especially striking and noteworthy is that even though there is no fully articulated doctrine of 'scientific method' until Aristotle, we find preliminary sketches, if no more, of various notions of how the investigation of natural phenomena should be conducted in several earlier writers, and here the evidence from the medical theorists is particularly valuable.[4] The extant texts of the period to the end of the fourth century B.C. provide, I believe, no example of the most extreme inductivist thesis, the idea that a theory or explanation can simply be *read off* from collected observations. Such a thesis is not stated, so far as I know, nor for that matter does it appear to have been assumed in practice by any early Greek scientist.[1] But one view that practising 'inquirers into nature' *do* express is a distrust of speculation that is not capable of being verified (even if their idea of what would constitute a verification may be rather crude). Historically the emergence of this idea may be associated with the reaction of certain medical writers against what they represent as an invasion of medicine and biology by the assumptions and methods of the cosmologists. The writer of *On Ancient Medicine* considered that the geological and meteorological theories of his contemporaries suffered from one fundamental flaw, there was no evidence to confirm or refute them, and the question of the methods proper to medicine and to cosmology is one that concerned other theorists at this period.[2]

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[1] *On the Nature of Man* (ch. 1) refers to the debates that took place on cosmological issues. The writer complains that given the same speakers and the same audience the same man never wins in the debate three times in succession.

[2] Cf. Popper's comments on the importance of different epistemological theories in antiquity (1963, pp. 164 ff., cf. 9 ff. and 236 ff.).

[3] By the end of the fifth century (or not long after) various positions had been stated by different writers on the question of the basis of knowledge, ranging from the pure intellectualism of Parmenides, to the crude sensationalism associated with Protagoras, via the complete scepticism that Gorgias professed: 'nothing exists, but if it exists, it is unknowable, and if it exists and is knowable, it still cannot be indicated to others' (*De Melisso, Xenophane, Gorgia*, 979 a, 12 f.). And that 'phenomena' or observed facts may be used as the basis for inferences concerning 'what is obscure' is the thesis of several writers beginning with Anaxagoras (Fr. 21a, see Lloyd, 1966, pp. 338 ff., 353 ff.).

[4] *On Regimen* I chapter 2, for example, states a view diametrically opposed to that of *On Ancient Medicine*: 'I say that he who intends to write correctly about human regimen must first know about the nature of man in general . . . , that is know from what things he is originally composed. . . .'

The medical writers provide, as we saw, some of the most remarkable early examples of the systematic collection of data, although their investigations were evidently carried out in the light of, and clearly reflect, their theoretical preconceptions. At the same time some of the Hippocratic authors had become aware that the problem of how to proceed in the inquiry into nature is *a problem* and took issue with the cosmologists over it. The dogmatic nature of much Presocratic speculation is a feature that had struck the writer of *On Ancient Medicine*, who explicitly rejected the use of arbitrary postulates such as 'the hot' and 'the cold'. It is clear to us that he too made certain quite arbitrary assumptions in his own pathological doctrines, for instance. But this does not diminish the importance of the fact that he attempted to distinguish the methods proper to medicine from those of cosmology. In judging the dogmatic elements of Presocratic thought, then, we should not forget that those very elements provoked a reaction among the medical writers who attempted to set up an alternative ideal of how the inquiry into nature should be conducted.

It is tempting to offer sweeping generalisations about the nature of science in antiquity, and particularly about where it differs from science as we know it today. But many of the favourite generalisations of the textbooks, helpful as they may be as very broad generalisations, often disregard quite a substantial body of evidence. It is often said, for instance, that Greek science is coloured, or rather tainted, by an all-pervasive teleology. Yet by the mid-fourth century, at any rate, the Greeks themselves were well aware of this issue, that is they could and did distinguish between different types of causes, 'teleological' or otherwise, and of course they took different sides on the substantive point, the atomists denying as strenuously as Plato and Aristotle maintained the relevance of teleological causation. Then it is often said that Greek science is unduly influenced by mathematical models, and again this may be true as a very broad generalisation, but again we should note that mathematics was not the only model for knowledge that was adopted. And I have argued elsewhere (Lloyd, 1964) that the failure of the Greeks to devise and carry out experiments has been greatly exaggerated.

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[1] The nearest the Greeks came to expressing such a thesis is, perhaps, the equation of knowledge with sensation associated with Protagoras: but one should note first that Protagoras himself was no scientist, no inquirer into nature, and second that we know this doctrine mainly from Plato (e.g. *Theaetetus*, 152a f.) and it may well be that Plato has exaggerated it for his own polemical purposes.

[2] Thus *On the Nature of Man*, chapter 1 also contrasts his own approach to the study of man with that of the cosmologists. Like the author of *Ancient Medicine* he criticises certain theorists for suggesting as the original constituents of man such things as air, or fire, or water, or earth, which do not obviously exist *in* man.

Generalisation appears equally hazardous on the question of the relation between theory-construction and empirical research in antiquity, as here too the situation varies from one branch of science to another, depending on the subject-matter investigated and the means available to investigate it. Thus if we take the chief question that preoccupied Greek natural philosophers in the earliest period, that of the ultimate constituents of matter, empirical investigations of the properties of different substances were almost certainly not undertaken at all extensively until the late fourth century, long after the theoretical debate had been initiated by the Presocratic philosophers. And in that instance the first such investigations that that we know of (the fourth book of the *Meteorologica*, Theophrastus's *De Lapidibus*) were the work of men who adopted the Aristotelian four-element theory as the basis of their inquiry.[1] In dynamics, too, it is evident that the first recognisable dynamical theories in antiquity (those of Aristotle) precede any attempt to conduct deliberate investigations to throw light on the factors which govern the speed of a moving object.[2]

In astronomy, however, the situation is more complex. First there is a series of rather crude models for the heavenly bodies (Anaximander's incorporates the idea that the fixed stars are lower, that is closer to the earth, than either the sun or the moon). But by the fourth century the Greeks certainly had access to the work of some Egyptian and Babylonian astronomers (see Aristotle, *De Caelo* 292 a, 7 ff.) and had already produced some observational astronomers of note themselves. In several cases we can trace how improvements in observational astronomy led to criticism of existing theories or indeed to new theoretical problems. The *Ars Eudoxi* provides information on the increasingly accurate estimates of the length of the seasons made from Euctemon and Meton in the late fifth century to Callippus in the latter part of the fourth. But this apparent irregularity in the movement of the sun constituted a difficulty for Eudoxus's theory of concentric spheres, and was, it seems,[1] one of the reasons that led Callippus to modify that theory in certain respects. Then to take an example from later Greek astronomy, it was by conducting his own detailed observations of the stars and by comparing these with those of earlier astronomers that Hipparchus was led to discover the precession of the equinoxes, and the observations in question (which are reported in Ptolemy, *Syntaxis* VII, chs. 1-2) need not have been, and probably were not, carried out with any idea of confirming or refuting a particular theory of the motion of the heavenly bodies.

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[1] This remains true whether or not it was Aristotle himself who composed *Meteorologica* IV.

[2] While Aristotle himself, so far as we can judge, seems, in his dynamics, to have relied almost entirely on common experience, we may take it from Simplicius, *in Ph.* 916, 12 ff., that Strato undertook certain tests in this field relating, for example, to the phenomena of acceleration.

In biology, some important problems, including, for example, the question of whether the seed comes from the whole of the body or not, had begun to be discussed before Aristotle. The so-called 'pangenesis' theory is associated with Democritus and may have originated with him, and it appears in the Hippocratic treatises *On Generation* (ch. 3) and *On Diseases* IV (ch. 32), which belong to the group of embryological works which also, interestingly enough, provide us with the first extant record of systematic investigations of the development of the hen's egg (*On the Nature of the Child*, ch. 29). But biology also provides some good examples of problems which were only recognised as problems after quite extensive research had been done, in particular after the method of dissection had begun to be used fairly widely. Thus it was only after dissections had been carried out on the heart that biologists began to discuss how precisely the blood moves into and out of the vessels of the heart and in the blood-vessels of the body generally.[2] And some of the typical problems debated by the Alexandrian biologists not only required the use of dissection and vivisection to be studied, but only emerged as questions to be investigated as a result of knowledge obtained in the first place by the use of those methods.[1]

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[1] This is the view of Eudemus reported in Simplicius, *in Cael.* 497, 18 ff.

[2] Of course even before dissection of the heart had been attempted, there are certain writers, such as Empedocles, who refer to movements of the blood in the body, but they do so in the vaguest terms. The first extant text to refer to the valves of the heart in the context of a description of its general structure is *On the Heart* (ch. 10) which also makes some obscure and fanciful suggestions about the contents of the various vessels of the heart. Thereafter the questions of the functions and contents of the arteries and veins and of the possible intercommunications between them were evidently debated by Herophilus and Erasistratus, although it is Galen that provides us with our first surviving texts in which these problems, together with that of the movement of the blood into and out of the vessels of the heart, are set out and discussed at length (e.g. *Nat. Fac.* III, chs. 14-15, Kühn II, 204 ff.).

These few examples already show (I suggest) that there are variations in the relationship between problem-appreciation, theory-construction and programmes of empirical research in different contexts in ancient science. In particular we can distinguish between programmes of empirical research (if we may call them that) that were fairly clearly undertaken with a view to providing data that would confirm or refute a particular theory, and those programmes that reflect not a desire to test a theory, so much as a desire to exploit a new scientific technique, such as dissection. Thus when the method of dissection was used systematically, this revealed problems that had not been dreamed of previously, and here it seems to me to make more sense to talk of the observations (not the problems themselves) as the starting-point of scientific progress. But whether or not that is accepted, I may end by observing that even a rapid survey such as this serves to indicate some of the quite striking differences to be found in early Greek science both in the conception of 'the inquiry into nature' and in its practice, and that so far as the historical issues arising out of the controversy between Popper and Kirk are concerned, what are needed are more, and more detailed, case-studies of specific scientific problems in antiquity, to throw further light on the intricate questions of the methods, and methodological assumptions, of early Greek scientists.

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[1] E.g. the investigation of the nervous system (both Herophilus and Erasistratus seem to have distinguished between motor and sensory nerves and to have attempted to trace particular nerves back to their origins) and the debate on the nature of digestion (where Erasistratus, having observed the peristaltic movement of the stomach and intestines, apparently maintained that this was solely responsible for the process of digestion). In both cases the investigation of the problem was taken much further by Galen, who provides the evidence concerning the work of the Alexandrians (e.g. *Loc. Aff.* III, ch. 14, *Nat. Fac.* III, ch. 4).

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On Thinking Machines and Feeling Machines*

by ROLAND PUCCETTI

I want to begin with the question whether machines can be said to think.

This is obviously not a straightforward empirical question like: 'Do Hottentots ever cry?' It is not something we know how to settle by making the appropriate observations. Rather it is a question like: 'Do cats really fall in love?', where the answer depends largely on agreeing to accommodate (or not to accommodate) the relevant facts to an established conceptual scheme. In this sense our question: 'Can machines be said to think?' involves a linguistic issue, not a factual one.

But the linguistic issue is complicated by the fact that until fairly recently no one would have raised this question. What makes the question philosophically pertinent today is of course the development of machines which function in ways normally associated with thought-processes. Unfortunately, these machines are familiar in detail and by direct contact to only a few people. One may expect, however, that with the refinement and proliferation of such machines, and their adaption to more mundane tasks than at present, the question will become more acute. By virtue of the same development, the linguistic issue involved in our question may *settle itself.*

What I mean here can be clarified by reference to a distinction pointed out by Putnam.[1] As he says, some linguistic issues need to be studied 'diachronically' and not just 'synchronically'. This is so not because the concepts at stake are changing, but because technological or scientific advances often provide new contexts for the old concepts. To use his example, the Greek equivalent of 'I am a thousand miles from you' could hardly have had a standard use (in interpersonal address) before the invention of writing. After that it *acquired* a standard use without changing the meaning of any of the words. What would have been, in Putnam's term, a 'deviant' Greek sentence then became nondeviant or normal.

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* Received 9.ii.66

[1] *Minds and Machines*, by Hillary Putnam, in *Dimensions of Mind* (ed. by Sidney Hook, Collier Books, N.Y., 1961), pp. 153-4. Future references to this volume will be abbreviated DOM.

Now with relation to the question: 'Can machines be said to think?', it seems to me an appropriate answer may be found by asking if the paradigm sentence, 'This machine is thinking', is likely to acquire a standard English use in the foreseeable future. Is it feasible, in other words, that extension of our present machine technology would render the sentence nondeviant? Or is the sentence a permanently deviant one?

I

The best way to consider how the sentence, 'This machine is thinking' might acquire a standard use is to take an example of a widely familiar, fairly complex machine and extend its functions to include some of the kind normally related to thinking. In doing this I shall be describing what is beyond contemporary technological achievement, but not beyond current capabilities.

Let us imagine an automobile, then, exactly like those we know except that it contains within it a mechanical unit physically analogous to contemporary computing machines. The addition of this unit does not alter our concept of an automobile, any more than installation of a radio, telephone, or air-conditioning system does. But it will make a striking difference in its performatory characteristics, because the automobile is now *self-directing* (for this reason I shall call the unit 'SDAD' for 'Self-Directing Automatic Driver').

We shall locate our SDAD unit in the present driver's seat, with suitable attachments to ignition, accelerator, brakes, gears, clutch, horn, steering wheel, and light switches. Its memory banks include a complete model of the city's streets, with all the necessary data for appropriate actuations in order to get the automobile from its present locus to any place corresponding to a point on the model. The stimulus for its operation can be a simple push on a button indicating where the owner of the car wants to go, but more sophisticated versions may respond to a verbal order like 'Sixteenth and Vine Streets, please!' The programing of SDAD will of course include reactions to driving situations in a modern city: for example, slowing down when approaching another vehicle going in the same direction; passing on one side only, provided no vehicle is coming from the opposite direction in the same lane, and so on. Its photo-electric sensors will allow the car to respond to conventional traffic signals of a mechanical or electrical kind, to come to a complete stop if a large object or hole appears ahead, etc. Re-programing will be necessary only when signals are changed or new streets opened.

So far none of SDAD's functions clearly carry over into paramental processes. Let us now imagine installation of a more advanced unit, called

'Super-SDAD'. The distinctive feature of Super-SDAD is that it has what Wiener[1] calls 'higher-order programing', i.e. it modifies its own basic programing in accordance with the results obtained in order to find better ways of achieving its present goals. Thus an automobile equipped with Super-SDAD is not only self-directing: it learns from experience and is able to solve unforeseen problems.

Suppose now we have two passengers in the back of a Super-SDAD car. The owner says into the speaker: 'Sixteenth and Vine Streets, please' They sit back and relax. But at the corner of Fourth and Vine the car comes to a slow stop. One passenger says: 'What's the matter? Isn't it working? Why doesn't it go on?' The owner replies: 'Wait a minute. *It's thinking.*' After a brief lapse the automobile changes gears, turns to the left and proceeds up Fourth Street in a northerly direction. 'You see,' comments the owner, 'it's going to try another route to escape that heavy downtown traffic.'

We have a fanciful situation here, obviously. It may never become economically practicable to substitute such complex artifacts as Super-SDAD for the human driver. But I do not think anyone can fairly say the situation I have described is technologically *impossible*. And if not, it is easy to see how, as machines of this higher logical type are refined and more widely adapted to human needs, the sentence: 'This machine is thinking' *could* acquire a standard use. (It may even become as common to distinguish 'thinking machines' from others as it now is to distinguish 'automatic' from less sophisticated machines). If this is so, if through technological change the old meanings of the words 'machine' and 'thinking' could determine a new use for a sentence combining them this way, then there is no fundamental conceptual incongruity in the claim that machines think.

Several objections to this argument may be anticipated. One is that even higher-programed machines like Super-SDAD would have their *overall* goals pre-set by human beings, hence do not think freely about them. This is true, but human goals are also pre-set by instinctual needs, childhood training, social conditioning, and so. (In the example I used the machine's goals hardly differ from the human driver's desire to get from one point to another in a congested city.) Another objection is that although Super-SDAD has great flexibility built into it, it is incapable of spontaneous, novel, or original self-direction because it has no 'free-will.' Leaving metaphysical prejudices aside, one must recognise that this is true in a trivial sense but false in an important one. Higher-order programing involves a 'randomising circuit' which makes complete predictability impossible in practice; that it should be possible in principle may be said with equal strength of human behaviour.[1] It is reasonably sure that an automobile equipped with Super-SDAD would not suddenly go off for a drive in the country when this alternative is not part of its program: but not more so than that a hired human chauffeur would.[2]

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[1] *The Brain and the Machine*, by Norbert Wiener, in DOM, pp. 109-11.

A more serious objection proceeds from the assumption that 'thinking' is an activity which must be identified with, or at least accompanied by, *consciousness*. Thus anyone who wants to claim machines can think must be willing to ascribe consciousness to them: which is supposed to be fantastic.

In his classic essay on the subject Turing[3] tried to counter this objection by arguing that the only way to know whether a machine is conscious is to *be* the machine, and either have or not have conscious thoughts. One can deny consciousness to other humans, he went on, on the same solipsistic grounds one denies it to machines. But of course there is no question of *knowing* whether a machine is conscious, only of whether we have as good reasons for ascribing consciousness to it as to other humans.

Let us grant for the sake of argument that we do not. Does this imply that machines cannot be said to think? It seems to me such a conclusion depends entirely on being willing to accept the premiss that consciousness is inseparable from thinking. And here two considerations are absolutely crucial.

In the first place, there are certainly *some* uses of words related to our concept of 'thinking' which have a purely descriptive function. For instance, when someone says: 'Be quiet! Can't you see he's thinking?', he does not mean you can *see* the other's thoughts. What he means is that you can see certain behaviour-patterns associated with ratiocinative performances. The same holds for 'thinking out a problem', where the 'thinking' is a series of calculations on a piece of paper. In both cases consciousness may be extrapolated, but it is not *entailed* by use of the word 'thinking'. One does not have to be a behaviourist to concede that.[4]

Secondly, while it may be (I do not think it is) that when referring to other humans' ratiocinative behaviour-patterns we would *always* be willing to go further and ascribe consciousness to them, it does not follow we need do this where machines are concerned. Again we must recognise that we are dealing 'diachronically' with this issue, and that it *is* a linguistic one. If, as seems technologically probable, we ever find ourselves in frequent contact with utilitarian artifacts exhibiting sophisticated ratiocinative processes, it would be perfectly natural to describe them as 'thinking machines'. Here we *need* not ascribe conscious thought-processes to them, on analogy with other humans, but I cannot see that it would make much difference if we did (more on this point later on). But if we simply refused to say such machines are 'thinking', how *else* should we describe their functions? To go back to the situation I described above, the automobile comes to a slow stop. Its Super-SDAD unit then does the following things: (1) it scans its memory store, (2) it recollects that at this time of the day this route becomes crowded with traffic, (3) it reviews its entire program for alternative routes to the same ultimate point on the model of the city, (4) it *selects* one of these in a scientifically unpredictable fashion, and (5) it then starts up the automobile again and directs it on to this new route. Now if we are afraid to say the machine was 'thinking' when doing these things, because of the association (not entailment) of 'thinking' with 'consciousness', we shall have to invent some entirely new word to describe Super-SDAD's behaviour.

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[1] See *The Compleat Robot: A* [*sic*] *Prologomena to Androidology*, by Michael Scriven, in DOM, p. 119 *passim*.

[2] Wiener has expressed concern that higher-programed machines will in fact come to escape human control (op. cit. pp. 111-12.)

[3] *Computing Machinery and Intelligence*, by A. M. Turing, in *Minds and Machines* (ed. by A. R. Anderson, Prentice-Hall, Inc., N.J., 1964). Future references to this volume will be abbreviated MAM.

[4] For the inadequacies, in my view, of a strictly behaviouristic interpretation of thinking, see 'Science, Analysis, and the Problem in Mind', by R. Puccetti, *Philosophy*, vol. xxxix, no. 149.

But why should we ever want to do this? We already have a perfectly good word—'thinking'—to describe what went on. We need only extend its present use to cover non-human instances of the same sort of behaviour, i.e. to new contexts where the old concept should be perfectly at home.

The curious reluctance of some philosophers to describe (presumably) unconscious ratiocination as 'thinking' can be illustrated by two examples. In his early study (he later went to the other extreme, as we shall see) Scriven[1] argued that computers cannot be called 'intelligent' because only *conscious* organisms deserve that adjective. Gunderson,[2] on the other hand, argued that although machines may be capable of *higher* intellectual achievements than men, we need not suppose they really 'think out' problems.

An answer to Scriven's early view of this, in line with what I have already put forward here, would be that when the paradigm sentence 'This machine is thinking' acquires a standard English use we should not find it at all strange to say machines are 'intelligent'. On the contrary it might then become a commonplace to say one machine is 'more intelligent' than another, because it achieves its overall goals more quickly and surely. As for Gunderson's disclaimer, what *would* superior problem-solving amount to, if not a better 'thinking out' of problems? Just because humans usually think out problems *consciously* does not imply (presumably unconscious) machines can not think them out too. Again, if we are not going to describe a machine's ratiocinative functions this way we shall have to coin a new word for the same thing. Perhaps we will want to do that some day, but we could hardly have a good *linguistic* reason for doing so; and in fact I believe that if we ever did this it would only be because of widespread human prejudice against machines.

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[1] *The Mechanical Concept of Mind*, by Michael Scriven, in MAM.

[2] *The Imitation Game*, by Keith Gunderson, in MAM, p. 71.

II

The possibility of some machines being conscious raises the conventional spectre of humanoid robots. I want to discuss this now, as a way of leading into the question of whether we need ascribe *feelings* to machines.

Suppose we imagine a group of computer scientists and assorted technicians resolved to play an elaborate practical joke on an exceptionally timid friend named Simon. In their spare time and in great secrecy they construct a mannequin of a beautiful young woman, complete with the usual feminine accessories. It has mechanical sensors connected with the mannequin's external 'sense-organs', a simulated respiratory cycle, heart-beats and pulse-beats, a special voice-production box, and so on. The head cavity contains a condensed digital computer with a storage capacity of 10^7 binary digits. Its program controls the above functions of the mannequin realistically, and includes a fairly large set of stock replies to stock questions. When the machine's interlocutor is identified as Simon, it actuates another, unusually warm set of responses. We now have a full-fledged lady robot. They give her the name 'R. Sally I.'

Our conspirators next arrange a special cocktail party, in the course of which shy Simon meets Sally for the first time. Her deference and eagerness towards him produce the expected result. After the party he goes about asking people who she is, trying to arrange further meetings with her. This success emboldens Simon's friends. They rehaul Sally and give her not only a larger storage capacity but a randomising circuit feature which will enable her to learn from experience with Simon and to adjust her future responses to unforeseen situations. The new program also includes tactual reactions of a well-defined kind. Renamed 'R. Sally II', the mannequin is then placed in a *boudoir* situation and an assignation with Simon arranged. To everyone's delight, the deception is again successful: Simon prevails.

But now a strange thing happens. Simon falls in love, he even talks of marriage in the spring. The conspirators become alarmed. What should they do? Should they tell him the truth, before it is too late? But what if he commits suicide? On the other hand, suppose he does marry her. If he never finds out, what harm will have been done? After all, Sally *is* learning. If she becomes difficult they can always change her program again. At this point, however, fate intervenes. Sally slips in the bathtub and fractures her skull. Simon finds her lying inert on the bathroom floor, broken transistors all about: the truth is out.

Let us imagine two quite distinct reactions to this. In Reaction A Simon goes to his friends and says: 'I forgive you for what you did, but you must help me now. I don't care if Sally *was* a machine. I was never so happy as when I had her. Please, please *put her back together again*!'

Now we can conceive Simon's friends being sympathetic to this reaction. They might also be repulsed by it, for it seems hardly healthy to want to continue a love affair with a machine, even one so accommodatingly programed as Sally is. But this is a *psychological* repulsion, not a *moral* one. For no one in this situation, not even Simon, believes Sally has any feelings. Thus it should not matter what Simon does with a reconstructed R. Sally II. Even if Simon were a secret sadist it would not matter, since no one supposes Sally can really be hurt, either physically or emotionally. She can only *seem* to be hurt; if Simon wants to live with an illusion, knowing all the time that Sally has no feelings, that could be of concern only with respect to the effects of this on his own personality.

But suppose Simon has a different, stronger reaction. In Reaction B he goes to his friends and says: 'I forgive you for what you did, but you must help Sally now. She was a fine and sensitive girl, so much in love with life. . . . Restore her happiness with me: please, please *put her back together again*!'

In this case Simon's friends might attempt to persuade him he has the whole thing wrong. They might point out that they *made* Sally what she seemed to be. But what can they say that would be absolutely convincing on this score? Suppose they say: 'Look here, Simon, she didn't *really* love you, or get pleasure from your embraces. We only programed her to *act* that way, even though in her second form she was able to review and modify that program. We could remake her tomorrow with a different program, so that every time you touch her she'll scream in agony. Don't you see she's just an intelligent doll?' Simon can *still* answer: 'Oh, don't do that. Don't cause her any pain. I want her to be happy with me just like she used to be.' Can his friends prove Simon wrong? Is there anything to show a highly complex, intelligent lady robot like Sally does *not* have feelings when her performance is parahuman?

In his later work Scriven[1] has argued forcibly that there is not. The view that because a machine is designed to say it is in love it cannot really be in love he regards as false, 'for the design may, and perhaps must, achieve both ends'. Performatory evidence for feelings is not decisive, he says, but it fulfils a 'necessary condition' of supposing them to occur. And what would be a sufficient condition? 'The answer must be that there is no *logically* sufficient condition statable in terms that can be verified by an external observer.' Thus if a computer-based physical system reports consciousness of pain, this may be accepted as true under certain conditions. The essential condition is that the machine be taught the language of feelings 'with great care', so that it possesses and knows to be relevant 'every statable proposition' involving sensations and emotions. Once this is achieved it would be 'wholly unreasonable', Scriven claims, to deny that it had feelings.

A lot depends here on what is understood by 'teaching a machine the language of feelings'. I have no doubt a machine could learn to use words referring to sensations and emotional states, as alternative ways of describing its internal changes. It may discover, for example, that 'I have a pain in my left shoulder' will do just as well as 'R. Sally II has an overload current at LX5612'; or that 'I'm getting bloody annoyed' will serve to express 'Pre-set goal Z cannot be attained under present conditions'. To maintain that having learned to do this justifies belief in the occurrence to the machine of pain or anger is, however, quite another matter. The human infant *first* experiences pain, anger, etc., and *then* gradually finds the words used in a public language to describe them. The feelings involved do not arise from learning the words, but from the organic experience those words refer to. So that before we can suppose learning the language of feelings is a proof of feelings we have to assume what Scriven's argument is designed to show: namely that the machine does have feelings.[2]

But do we have to assume that? I suppose many philosophers would answer: 'Yes, if you're going to ascribe thinking to a machine there's no good reason to withhold feelings, given a suitable design. They go together in humans, so why not in a parahuman machine?' The attitude seems to be that this an all-or-nothing proposition, that no logical wedge can be driven between thinking and feeling, and that one must either deny both or accept both.

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[1] Namely, in *The Compleat Robot: A* [*sic*] *Prolegomena to Androidology* by Michael Scriven, in DOM, pp. 130-3; and in his 1963 Addendum to the earlier article, *The Mechanical Concept of Mind*, in MAM, p. 42. See also *Computers and Comprehension*, co-authored with M. Kochen, D. M. MacKay, M. E. Maron, and L. Uhr (Memorandum KM-4065-PR, Rand Corporation, Santa Monica, California, April 1964), pp. 2-3 and 6-7 especially.

[2] Scriven's collaborators in *Computers and Comprehension* seem to acknowledge this when they admit in a footnote that although a blind man may know the statement 'Grass is green' is normally true, it 'would be less plausible to claim either a blind man or a computer knows what is *meant* by saying that' (op. cit. p. 7).

A good example of this attitude may be found in MacKay's paper, *The Use of Behavioural Language to Refer to Mechanical Processes*.[1] Early in that paper he supplies a 'semantic chart' intended to help us sort out concepts and relationships involved in any comparison of artificial and natural entities, which I reproduce here.

NATURAL		ARTIFICIAL	
Personal aspect	Mechanical aspect	Mechanical aspect	Personal aspect
	Growth	*Construction*	
Person	*Brain-and-Body*	*Automaton*	?
(Joe)	('Mass of *cells* and *things*')	('Mass of *wires* and *valves*')	?
	Carrying *signals*	Carrying *signals*	
	Forming an *information-system*	Forming an *information-system*	
	Organising observable *behaviour*	Organising observable *behaviour*	
	indicative of	*indicative* of	
Thinks, feels, hopes, fears	Thinking, feeling, hoping, fearing . . .	Thinking, feeling, hoping, fearing . . .	?

The structure of this chart is suggestive: it indicates how, if the analogy between the two inner columns is strong enough in any given case, one may reasonably pass from the inner to the outer column on the right-hand side just as we do on the left-hand side. All that is required, says MacKay, is a 'commitment' to use personal language towards the artificial entity as one does towards natural entities. MacKay confesses he does not himself feel inclined to make that commitment with existing automata, but as he reminds us the question for philosophers is whether it would be ***absurd*** to do so in all circumstances. Evidently he does not think so.

My objection to this presentation of the problem proceeds from lumping together at the bottom of the chart 'thinking' and 'feeling'. As I tried to show in the first section of my paper, one can legitimately pass from certain kinds of behaviour in a machine to a claim that the machine "thinks": whether this be understood to merely describe that behaviour or, more controversially, to attribute conscious thought-processes to the machine. And what Professor MacKay's chart really shows, in this connection, is that 'thinking' is something independent of the entity's structural components and origins.[1] But can we say the same for 'feeling'? Surely not. What is at the top of the inner columns in MacKay's chart makes an enormous difference to deciding whether behaviour *indicative* of feeling really does involve feeling-states. For what is odd about Simon's 'Reaction B' is just that he supposes Sally did feel pleasure, love for him, etc., after finding out she had been *constructed* to behave this way, and that she was *made of* 'a mass of wires and valves'.

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[1] D. M. MacKay, *The Use of Behavioural Language to Refer to Mechanical Processes*, this *Journal* 50 (1962), 89-103.

Suppose Sally had behaved as if hungry. But she has no stomach. At most she could have the mechanical equivalent of a stomach, not one component of which (if it is really a *mechanical* stomach) is or ever has been related to the biochemical process of ingesting, digesting, and assimilating nourishment from food. Or suppose Sally had behaved as if in lust. But she does not have the glands, the reproductive organs, and so on, related to sexual desire. What Simon's friends know, and would try to make clear to him, is that such feeling-states arise in the context of a larger story, a story which has a place for Simon but none for Sally. It is the story of organic evolution, of the gradual acquisition of sensory equipment and organs requisite to survival in a competitive biological environment. Sally's components are by their proper designation outside that story; no matter how ingeniously they are organised to *simulate* biochemical processes, they cannot suddenly become part of this story.

'Growth', 'brain-and-body', 'mass of cells and things': these are *not*, as MacKay's chart misleadingly labels them, 'mechanical' aspects of a human being, or any other 'natural' entity. It is in recognising this that we see the absurdity (though not the logical impossibility) of ascribing feelings to a robot, even if it had a mechanically-constructed anatomy precisely analogous to a human's. True, some parts of the human body can be replaced by mechanisms without undermining our belief that the subject has feelings. Perhaps, as MacKay suggests, a human's *brain* might be replaced, gradually, by an identically-organised 'artificial brain' without affecting the logic of the situation (the brain is not itself sensitive). But such a man would not be an artificial person; he would be a natural person with an artificial brain. The difference is like the difference between a man with, say, an artificial lung and one with a glass eye. The former can breathe with it, but the latter cannot see with it. And a parahuman automaton like Sally, though she can 'function' as a living person does, still has no feelings because when all is said and done, nothing in her composition ever did or could develop those *biological* functions which give rise to sensations and feeling-states.

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[1] Fred Hoyle has drawn a convincing picture, I think, of how even an interstellar *cloud* might be pronounced 'intelligent' (cf. his science-fiction novel, *The Black Cloud*, Heinemann, 1957). To be strictly accurate, however, the cloud is also described as living.

There is another oddity in Professor MacKay's semantic chart which must now be brought out. All of the right-hand side is labelled 'artificial', as if mechanical entities (the only ones he deals with) were the only possible artificial ones. But this is not so. Without going into any detail here, it is obvious that one could, theoretically, *construct* an artificial agent of natural components: a 'mass of cells and things'. The resultant organised information-system would not be a natural entity, because constructed rather than grown, but behaviour by it which is indicative of feeling *could* certainly allow extrapolation to feeling-states.[1] If Simon's friends had made Sally that way, she could then learn the language of feelings the way human children do. We should in that case have no good reason to doubt she means what a human does when she says: 'Don't do that Simon, you're hurting me!' And if so, if Sally were a *feeling artifact*, then it *would* involve moral considerations to turn her over to Simon for his pleasure. But then she would not be a robot at all. She would be an extra-human person.

This brings me to a final observation on Professor MacKay's presentation of our problem. In the course of his paper he suggests a chess-playing automaton may come to be counted a person if we agree to play against it. For just as we do not play against a human's *body*, or even his brain, so we would not be playing against the 'mechanism', but against *him*. To speak of playing a game against the automaton is to personalise it. And this is not, MacKay insists, to jump a step. We do not have *first* to discover 'whether a chess-playing mechanism has a personality, or is nothing but a mass of wires and valves'.[2] The decision on the issue of personality is implicit in the decision to play a game of chess with this entity.

Now it is true that a computer-based chess-playing machine would 'reveal a personality' in play: even roulette wheels do that. But 'personality' in this *weak* sense is not what MacKay means. To judge by what he says, he means 'personality' in the *strong* sense of revealing one's 'person-status'. But I do not think for one moment that playing against a chess-playing machine (something I look forward to, actually) involves any commitment to regard it as a person. For as I have been at pains to argue, I can count an entity a 'person' only when I believe that entity has feelings, can really be hurt or made happy, the way humans do and can. Because

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[1] I do not think this possibility conflicts with what I said earlier about 'construction' in relation to mechanical entities. For although deliberately put together, this artificial entity is made of elements which themselves have a place in the larger 'story' of natural evolution.

[2] Op cit. p. 98.

R. Sally II had no feelings, she was not a moral object: this is where Simon went wrong. But a new Sally, constituted of organic materials, *could* be credited with feelings. If she were also capable of rational activities —like learning human language and playing chess—she would certainly have acquired person-status. The crucial difference between MacKay's stand on this and mine may be expressed very simply. If one loses a game against a chess-player which is a chess-playing *mechanism* and, in anger, smashes the machine, would this not have very different moral and legal implications than if the chess-player could be believed to have feelings? The real question is not: 'Does this chess-playing automaton have a personality?'; but rather: 'Does this chess-playing entity, *being an automaton*, have feelings, so that I can count it a person?', and here the answer must be negative if it is truly a machine.

On the issue of feelings, then, we seem to have reached a logical limit to machine technology. A machine can think, be intelligent, arrive at original and unforeseeable solutions to problems, modify its responses in accord with experience, assimilate human language—including the language of sensations and emotions—and even be self-reproducing.[1] But so long as it is a *machine* it will not have feelings, precisely because its components belong to another order of nature than the organic. This is why the phrase 'feeling machine' seems permanently deviant, while 'thinking machine' is not.

III

By way of concluding I want now to say why I think much of the contemporary debate on 'mentality in machines' has gone awry. On the one side we have philosophers like Scriven and MacKay who, conversant with the possibilities of machine technology, see no reason why one should not say machines can think. But because the blanket term 'mentality' easily lets in associations—in *human* mental life—such as 'consciousness', 'feeling', 'emotion', 'sensations', and 'personality', they have been tough-minded enough to go the whole way. For exactly the same reason, more conservative (at least on this issue) philosophers have gone to the opposite extreme and attempted to exclude mentality from machines even in the limited sense of being originally and interestingly ratiocinative, as advanced computers certainly are.

What we need to do to break down this (in my view) wholly unnecessary division into hostile camps is to abandon the useless concept 'mentality'.

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[1] John von Neumann demonstrated the theoretical possibility of mechanical self-replication in his 1951 essay, *The General and Logical Theory of Automata*. There is no question here of *feelings* associated with organic reproduction.

The first step in doing that is to recognise, as I have tried to show above, the distinction one may legitimately draw between 'thinking' and 'conciousness': so that on the narrower meaning no one need deny machines 'can be said to think' in order to deny them consciousness.[1] But more important still, we must recognise that consciousness in ratiocinative mechanisms, unlike *feeling* entities, has no moral implications in itself. For as I have said it is only the conjunction of sentience, emotional states—in a word, *feelings*—with ratiocination which yields the concept of a person. Destruction of an artificial entity possessing both feelings and rational thought-processes has important moral implications, on a par with destruction of a human. But where there is no question of feelings being involved, as in a machine, there is no question of person-status either. Thus to destroy a true machine, *even if it thinks consciously*, amounts to no more than arresting its conscious thoughts, which so far as I can see has no moral implications whatsoever. What makes a human's death tragic, as any person's death, is not so much the cessation of conscious thought as the loss of all feeling, all emotion, all sensation. It is this we mourn, and cannot reasonably expect in the destruction of a machine. So that what really cuts a machine off from the community of persons is not a necessary lack of consciousness, but (given its structural components) a highly probable lack of feeling.

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[1] I have been deliberately neutral, in this paper, on the question of whether thinking machines *are in fact conscious*. For ways in which a machine might use conventional arguments to show *humans* do not (consciously) think, see my article 'Can Humans Think?', *Analysis*, 26, 1966.

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Discussions

BAUMER ON THE CONFIRMATION PARADOXES

In a recent article[1] William H. Baumer presents a solution to the paradoxes of confirmation which makes use of his eliminative theory of confirmation. I will argue in this paper that his solution is not satisfactory.

The precise formulation of Baumer's eliminative confirmation criterion is contained in his article. But for purposes of this paper, the following rough version will do:

(A) Given a finite set of properties, $F,G,H, \ldots, M$, such that (1) all of its members, G included, have been observed present with F in some instance, and (2) absent with F in another, as well as (3) absent in F's presence in none, then (4) if an instance is found in which F and G are present, but at least one property $H, \ldots, M$ is absent, then that instance confirms (i.e. increases the probability of) G as a necessary condition of F.

(B) Similarly, given a finite set of properties, $F,G,H, \ldots, M$, such that (1) and (2) above have been satisfied, and (3) if there are no known instances of the presence of F, or H, or . . . , or M, in G's absence, then (4) if an instance is found in which F and G are absent, but at least one property $H, \ldots, M$, is present, then that instance confirms F as a sufficient condition of G.

1. *Baumer's Solution to the Confirmation Paradoxes.* (*a*) Baumer believes that the confirmation of an hypothesis such as (I): ‘$(x).Fx \supset Gx$’ by what he calls *d* instances (such as $\sim F \sim Gd$) seems paradoxical because (usually) there are so many such instances readily at hand. And he admits that any theory which permits *all d* instances to confirm (I) is unsatisfactory. He claims to have solved this problem, which he calls the *third* confirmation paradox, because his theory strictly limits the number of *d* instances which can confirm (I). Indeed, it strictly limits the number of *d* instances which can confirm (II): ‘$(x).\sim Gx \supset \sim Fx$’, as well as the number of what he calls *a* instances (such as FGa) which can confirm (I) or (II). So his theory does not permit vast numbers of *d* instances to be chalked up in order to highly confirm (I), as do the usual instantial theories. Moreover, Baumer claims that all of the instances which are confirming on his theory are intuitively confirming.

It is also worth noting that his theory does not permit empty confirmations, such as the confirmation of the hypothesis ‘All Martians are pink’, solely by *d* instances, since his theory requires evidence of at least one *a* instance and one *d* instance before *any* instance is confirming.

Finally, notice that there is a sense in which Baumer's criterion does not violate the so-called ‘equivalence criterion’ (which requires that whatever confirms a given statement must equally confirm all logically equivalent statements) since any *d* instance which confirms (II) also confirms (I).

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[1] ‘Confirmation Without Paradoxes’, this *Journal*, 1964, **15**, 177-95.

But the *way* in which the equivalence criterion is satisfied is important. Consider the evidence (i): $FG{\sim}Mc$. According to Baumer's theory, (i), in conjunction with other appropriate evidence, confirms (I), in the sense that it confirms G as a necessary condition of F, and also confirms the logically equivalent hypothesis (II): '$(x).{\sim}Gx \supset {\sim}Fx$', in the sense that it confirms ${\sim}G$ as a sufficient condition of ${\sim}F$. Thus both (I) and (II) are confirmed by the evidence (i), and the equivalence criterion seems to be satisfied. Similarly, the instance (ii): ${\sim}F{\sim}GMd$ not only confirms (I), in the sense that it confirms F as a sufficient condition of G, but also (II), in the sense that it confirms ${\sim}F$ as a necessary condition of ${\sim}G$.

(*b*) Baumer's theory solves what he calls the *second* confirmation paradox (e.g. the counterintuitive confirmation of (I) by *b* instances, such as ${\sim}FGb$, ${\sim}FGMe$, etc.) simply by rejecting all *b* instances as confirming instances. His theory rejects them because the fourth section in part (A) of the eliminative criterion requires the presence of both F and G, but F is absent in all *b* instances, and the fourth section of part (B) requires the absence of both F and G, but G is present in all *b* type instances.

2. *Objections to Baumer's Solution.* (*a*) The problem Baumer calls the second confirmation paradox does not arise simply because *b* instances seem intuitively unacceptable as confirming instances for hypothesis such as (I) and (II). It is also generated by the fact that the same *b* instances which seem intuitively unacceptable as confirmations for (I) and (II) appear to be perfectly acceptable as confirmations for the logically equivalent hypothesis (III): '$(x).{\sim}Fx \vee Gx$'. So if we reject *b* confirmations for (I) and (II), we must do so also for (III), which seems unacceptable, while if we accept them as confirming for (III), then we must do so also for (I) and (II), which seems equally unacceptable. Either way, violence is done to our presystematic intuitions.

(*b*) Baumer's particular brand of eliminative confirmation contains several features which are hard to accept:

(1). The plausibility of an *eliminative* theory of confirmation rests on the idea that in order to confirm a given hypothesis one must eliminate one of its *competitors*. But on Baumer's theory it is possible to confirm *without* eliminating genuine competitors. For example, if F is the property of being a raven, G of being black, and M of being male, then the elimination of M as a possible necessary condition of F is not the elimination of a genuine competitor of G as a necessary condition of F, since all ravens could be both black *and* male.

(2). Baumer's theory fails to allow for the confirmation of any hypothesis not equivalent to some hypothesis of the form '$(x).Fx \supset Gx$' or '$(x).Fx \equiv Gx$'. But this gives rise to the difficulty that a given statement, S, may be confirmable, while its negation ${\sim}S$ is not. For instance, the hypothesis (I) is confirmable on his theory, while its negation '${\sim}(x).Fx \supset Gx$' is not. But surely an instance which eliminates (I) must at the same time confirm its negation.

(3). Baumer's theory arbitrarily makes eliminative instances confirm certain hypotheses while not confirming others. For example, under certain conditions his theory allows instance (i): $FG{\sim}Mc$ to confirm G as a necessary condition of F, because it eliminates M as a necessary condition of F, while not allowing (i) to confirm F as a sufficient condition of G, even though (i) eliminates F as a sufficient condition of M. Similarly, his theory allows instance (ii): ${\sim}F{\sim}GMd$

to confirm F as a sufficient condition of G, while not allowing (ii) to confirm G as a necessary condition of F, even though (ii) eliminates G as a necessary condition of M. I'm afraid I fail to see the difference between these cases which makes a difference.

(4). There are other reasons for rejecting this important feature of Baumer's theory.

(*a*) In order not to violate the equivalence principle, Baumer must hold that, say, the statement (IV): 'G is a necessary condition of F' is equivalent to the statement (V): 'G is a sufficient condition of F', while *denying* that (IV) is equivalent to the statement (VI): 'F is a sufficient condition of G'. To me this seems to be arbitrary and unacceptable.

(Baumer claims that his theory does not violate the equivalence criterion, since, for example, any instance which confirms hypothesis (I) equally confirms the equivalent hypothesis (II). But this neglects his requirement that we construe hypotheses as stating either a necessary or a sufficient condition of some property, but not both. If an instance can confirm (I) construed as stating (IV), but not confirm (I) construed as stating (VI), and if one accepts the view that (IV) and (VI) are equivalent, then one must conclude, as I do, that Baumer's theory *does* violate the equivalence criterion.)

(*b*) Baumer is not unaware that this peculiar feature of his theory might be found objectionable. He remarks that '. . . an acute observer might detect a bit of an oddity, and ask, rhetorically, whether or not (I) as a necessary condition statement is equivalent to (I) as a sufficient condition statement',[1] and he furnishes two arguments in defence of his view on this point.

His first argument is that if we have an evidence set such as (iii): $FGMa$, and (iv): $\sim F \sim G \sim Mb$, and (v): neither G nor M has been eliminated as a necessary condition of F, then it is easier to construe (I) as stating that G is a necessary condition of F. But we *could* construe it as stating that F is a sufficient condition of G. *However*, if we do so, then since the information at our disposal *is* the evidence (iii), (iv), and (v), '. . . we are not as it were searching for the sufficient condition of G; rather we are looking for that property of *which* F is a sufficient condition. But this is all one with looking for a necessary condition of F'.[2]

I find this totally unsatisfactory.

(1). Many philosophers, including myself, would claim that what a hypothesis means should not depend on what evidence one happens to possess.

(2). How does it follow that if the evidence at our disposal is the set (iii), (iv), and (v), and if we construe (I) as stating that F is a sufficient condition of G, that we are not searching for a sufficient condition of G but rather for a property of which F is a sufficient condition? Remember, even Baumer has to admit that if we start with the evidence set (iii), (iv), *and* (vi), where (vi) states that neither F nor M has been eliminated as a sufficient condition of G, then we *are* searching for a sufficient condition of G. What is the magic difference between (v) and (vi) which influences us to search for one thing rather than another?

(3). If the evidence at our disposal happens to be (iii), (iv), (v), *and* (vi), then presumably Baumer must say that we are searching both for a sufficient condition of G and a necessary condition of F. And yet according to his theory, in such a case the new evidence (i): $FG{\sim}Mc$ is confirming for G as a necessary condition of F but *not* confirming for F as a sufficient condition of G.

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[1] 'Confirmation Without Paradoxes', this *Journal*, 1964, **15**, pp. 180-1.

[2] Ibid. p. 181.

(4). But then to me the whole concept of *searching for something* does not belong in a theory of confirmation. Baumer seems to be overlooking the difference between what some call the contexts of *discovery* and of *justification*.

(5). Baumer's second defence of his view about necessary and sufficient conditions is that '. . . confirmation of (I) relative to (iii), (iv), and (v), and some instances is confirmation of (I) relative to different evidence than is confirmation relative to (iii), (iv), and (vi), and some instances'. He claims that 'There is *no* problem in having the same (or equivalent) hypotheses having different degrees of confirmation relative to various bodies of evidence'.[1]

But the objection to Baumer's theory is not that it requires equivalent hypotheses to be confirmed to different degrees by different evidence, but rather that it requires equivalent hypotheses to be confirmed to different degrees by the *same* evidence, as when it allows instance (i) to confirm G as a necessary condition of F, but not F as a sufficient condition of G. Baumer's reply says nothing about this objection.

(*c*) Finally, we must deal with Baumer's solution to the third confirmation paradox.

(1). His theory does not permit the confirmation of hypotheses such as 'All Martians are pink' unless at least one pink Martian has been observed. But suppose we send a space ship to Mars, and observe *exactly one* living Martian who just happens to be pink. Then, according to Baumer's theory, although this instance of a pink Martian is not confirming for the hypothesis 'All Martians are pink' (itself an objection to Baumer's theory), we can highly confirm that hypothesis quite easily without ever observing another Martian, simply by observing appropriate instances of non-pink non-Martians. Baumer's reply that only a few such cases are possible seems to me to be incorrect, as I argue below. But even if it were correct, it would not answer the objection, since (as I also argue below) the few such cases would be highly confirming on his theory.

Further, if I am right that it makes no sense to say that one property is confirmed as a sufficient condition of a second without at the same time allowing that the second is confirmed as a necessary condition of the first, then *all* of the cases of non-pink non-Martians which confirm Martianhood as a sufficient condition of pinkness must also confirm pinkness as a necessary condition of Martianhood. Thus the third confirmation paradox still would be with us.

(2). Suppose we have initial evidence stating that a set of properties $F,G,H, \ldots, M$, are present and absent in the required way, and then obtain evidence which eliminates $H, \ldots, M$, as possible sufficient conditions of G. Now suppose we link a new set of properties, $N, \ldots, P$, with F and G in the required way, and assume we cannot determine whether $N, \ldots, P$ were present or absent in the original instances which eliminated $H, \ldots, M$. Then, interpreting Baumer's theory literally, a new instance which eliminates, say, N, as a sufficient condition of G does *not* increase the confirmation of F as a sufficient condition of G, since N was not a member of the original set of characteristics, $F,G,H, \ldots, M$. Surely this is not satisfactory on an eliminative confirmation theory.

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[1] 'Confirmation Without Paradoxes', this *Journal*, 1964, **15**, p. 181.

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(3). Suppose we amend Baumer's theory so as to remove this difficulty by allowing instances which eliminate $N, \ldots, P$ to confirm F as a sufficient condition of G (as well as confirming G as a necessary condition of F). Then the third paradox would remain unsolved, since we would be able to chalk up very large numbers of counterintuitive *d* type confirmations of F as a sufficient condition of G. (The situation would be similar for confirming G as a necessary condition of F.)

Baumer cannot reply to this objection by claiming that only a few counterintuitive *d* type instances would confirm in this way,[1] for even a few would be too many. And more importantly, it does not seem to be the case that the number would be small. As Nelson Goodman (among others) has pointed out, the number of ways in which any set of instances both resemble each other and differ from each other is indefinitely large. Consequently, there are always a great many competing hypotheses to be eliminated, and not just a few.

(4). But suppose I'm wrong and the number of possible *d* type confirming instances in small, as Baumer claims. Then the number of possible *a* type confirming instances must also be small, perhaps even smaller than the number of possible *d* type instances. But if so, then the paradoxical *d* type instances will count at least as heavily as genuine *a* type confirmations, and Baumer's theory (as amended) will permit the very consequences (such as the possibility of 'indoor ornithology') which constitute the third paradox of confirmation, so that his solution to the paradox will be no solution at all.

The confirmation paradoxes remain unsolved.

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[1] In fact, his reply is that none of the confirmations allowed are counterintuitive. But since he offers no reasons for thinking so, all I can say is that they *do* appear counterintuitive to me.

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PARTICLE AND FIELD THEORIES OF GRAVITATION

I

Einstein has recorded[1] that his relativity theory of gravitation arose from reflection on what appeared to be the remarkable coincidence, unexplained on Newton's theory, that the 'gravitational mass' of a body was always exactly equal to its 'inertial mass', and the fact that on Einstein's theory this equality is *necessary* is often cited[2] as evidence of its epistemological superiority to Newton's. The purpose of this note is not to attempt a final judgment between the theories, or fully to evaluate either, but simply to point out, first, that a comparison on this ground is unjust to Newton and becomes possible only when his theory is distorted into a form that makes it appear open to criticism from which it is actually quite immune; and second, to consider more generally the characteristics of particle and field theories, of which Newton's and Einstein's theories of gravitation are outstanding examples.

I do not know when the supposed distinction between 'gravitational mass' and 'inertial mass' was first made, but it is not to be found in Newton's *Principia*. He introduces only one such quantity—*mass*. Its measure ('arising from density and bulk conjointly') has been re-defined since to accord better with later formulations of the theory, but this need not concern us because all expressions of it agree with Newton's in that they are independent of any reference to gravitation—i.e. they relate only to the so-called 'inertial mass'. He goes on to say: 'It is this quantity that I mean hereafter *everywhere* under the name of body or mass' (my italics) and he continues: '*the same* is known by the weight of each body, for it is proportional to the weight, *as I have found by experiments*' (again my italics). In other words, he had made the experimental discovery—not defined a new quantity—that two independently measured quantities were proportional to one another.

There is nothing in this more (or less) mysterious than any other experimentally discovered relation—e.g. the relation between heat and work; it is just an example of the ordinary course of scientific discovery. It is a particular case of the more general theory of gravitation that Newton then proceeds to expound. In his first law he lays down the principle that he is going to regard the motion of any body unacted upon by a force as *uniform* (in modern language, he chooses an inertial co-ordinate system), and then it follows logically that a body whose motion is not uniform must be acted upon by a force. It is a matter of experience that two apparently free bodies (e.g. the Earth and a falling apple) do not both move uniformly, whatever co-ordinate system is chosen: hence a force must act on at least one of them, and the third law stipulates that the same force acts on both. The force is proportional to the rate at which it changes the motion, and we can therefore evaluate it by measuring that rate.

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[1] E.g. in *Albert Einstein, Philosopher-Scientist* (Library of Living Philosophers), 1949, pp. 64, 65.

[2] E.g. by M. Born, *Einstein's Theory of Relativity* (Dover), 1962, p. 44.

All this, however, takes us no further in the direction of finding relations between the various natural phenomena. We simply discover empirically that there is an acceleration, and of course it must have some magnitude. The achievement of Newton was that he showed that that magnitude could be *predicted* by measuring quantities independent of gravitation—namely, the masses and initial separation of the bodies concerned. His theory therefore established a general relation between two, at first sight, quite distinct phenomena now known as *gravitation* and *inertia*—a relation confirmed (at least to a high degree of approximation) by numerous subsequent experiments and observations.

What is there in this to suggest an unexplained coincidence in any but the perfectly general sense that all relations between phenomena are unexplained coincidences? The heating effect of the conceptual entity that we call an electric current is found to be proportional to its effect on a neighbouring magnet; but we regard that as an important piece of knowledge, not as a defect of electrical theory which contains an unexplained identity of heating current and magnetising current. Why should the discovery that the accelerations of neighbouring bodies are related to their masses be regarded as a defect of the theory giving that relation?

The answer to this question, I think, lies in the fact that when Newton's theory was later translated into the form of a field theory, new conceptions were introduced between which an arbitrary equality had to be assumed in order to preserve the legitimacy of field theory as a possible expression of Newton's particle theory. 'Gravitational mass' and 'inertial mass' had to be recognised as separate entities *only* in order to make it possible to describe Newton's theory in field terms, and Einstein's identification of them was not the removal of an arbitrariness in Newton's theory, but the removal of an arbitrariness in the previous translation of that theory into the field form. If you first of all decide that only a field theory of gravitation is permissible, you can admit Newton's theory only in a form that involves two independently defined quantities where he required only one, and then a field theory, such as Einstein's, that makes these quantities *necessarily* equal appears preferable to one in which their equality is only *accidental*. But if you make no such initial decision, but seek only the theory, whether field or particle, that best decribes the phenomena, then Newton's theory, so far as this point is concerned, is at least as good as Einstein's, and the decision between them (apart, of course, from the conclusive one of their accordance with observation, on which ground at present the difference is so slight and uncertain [1] that the only wise course is to suspend judgment) must depend on the more fundamental decision as to whether a field or a particle theory is more satisfactory for describing nature as a whole, including electrical, magnetic, thermal—and possibly ultimately vital—phenomena as well as mechanical.

The violence done to Newton's theory by translating it into field form is shown very clearly in Einstein's account of the matter, given in its most extended form, I think, in his book, *The Theory of Relativity* (Methuen, 1920). Though a popular work, there is no lack of rigour in this presentation. Einstein, unlike some popularisers, never wrote loosely to evade intellectual difficulties, and indeed it is only in such writings as this that one can find a detailed account of the general conceptions underlying the mathematical treatment of these problems. Einstein here introduces the field concept by the example of magnetic attraction, in which the direct action of a magnet on a piece of iron is abandoned in favour of, first, the creation of a magnetic field by the magnet, and second, the action of this field on the iron. He proceeds (p. 63): 'We shall not discuss here the justification for this incidental conception, which is indeed a somewhat arbitrary one. We shall only mention that with its aid electromagnetic phenomena can be theoretically represented much more satisfactorily than without it.' He then goes on: 'The effects of gravitation also are regarded in an analogous manner. The action of the Earth on the stone takes place indirectly. The Earth produces in its surroundings a gravitational field, which acts on the stone and produces its motion of fall.' Applying this to Newton's gravitational theory, he says:

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[1] See, for example, *Proc. Roy. Soc.*, A 270 (1962), p. 297.

> 'According to Newton's law of motion, we have
>
> $$\text{(Force)} = \text{(inertial mass)} \times \text{(acceleration)},$$
>
> where the "inertial mass" is a characteristic constant of the accelerated body. If now gravitation is the cause of the acceleration, we then have
>
> $$\text{(Force)} = \text{(gravitational mass)} \times \text{(intensity of the gravitational field)},$$
>
> where the "gravitational mass" is likewise a characteristic constant for the body. From these two relations follows:
>
> $$\text{(acceleration)} = \frac{\text{(gravitational mass)}}{\text{(inertial mass)}} \times \text{(intensity of the gravitational field)}.$$
>
> 'If now, as we find from experience, the acceleration is to be independent of the nature and the condition of the body and always the same for a given gravitational field, then the ratio of the gravitational to the inertial mass must likewise be the same for all bodies. By a suitable choice of units we can thus make the ratio equal to unity. We then have the following law: The *gravitational* mass of a body is equal to its *inertial* mass.
>
> 'It is true that this important law had hitherto been recorded in mechanics, but it had not been *interpreted*. A satisfactory interpretation can be obtained only if we recognise the following fact: *The same* quality of a body manifests itself according to circumstances as "inertia" or as "weight" (lit. "heaviness").'

From this, I think, it is quite clear that what Einstein's theory of gravitation, so far as this point is concerned, claims to improve is not Newton's theory but the previously existing field expression of Newton's theory. The fact that the same quality of a body manifests itself as inertia or weight is exactly what Newton had discovered experimentally, as he stated in the passage from the beginning of the *Principia* quoted earlier. Einstein's theory merely expresses that fact differently.

It is of some interest now to see how far the translation of Newton's theory into field terms (irrespective of the particular field form chosen) is legitimately possible. First of all, we notice, as an essential characteristic of Einstein's translation, that it divides what is originally a single process into two independent processes. In Newton's theory there is a single attraction between two separated bodies, A and B, which accelerates them towards one another. In the field theory, A first of all creates a field and then B reacts to that field. But to make the description complete we must add that B also creates a field and A reacts to B's field. This is not merely a different expression of the former statement; it is a necessary supplement to it. The complication introduced by this translation is therefore considerable, and it is not surprising that it needs some arbitrary hypotheses to sustain it.

Consider two masses, M_1 and m_1, held at rest at a distance r apart (in any co-ordinate system you please if you wish to think in terms of co-ordinate systems). Now release them at the same moment. A force M_1m_1/r^2 (assume $G = 1$) acts on each, producing in M_1 an initial acceleration m_1/r^2 and in m_1 an initial acceleration M_1/r^2. These have perfectly definite values, and can in principle be measured. Now substitute m_2 for m_1 and repeat the process. Then a force M_1m_2/r^2 acts on each, producing in M_1 an initial acceleration m_2/r^2 and in m_2 an initial acceleration M_1/r^2. It appears, then, that the reaction of a body to a field (the acceleration produced in it by the field) is independent of the mass of the body, but the field produced by a body is not independent of its mass. If we are going to define a mass in terms of gravitational properties, independently of Newton's definition in terms of inertia, we should therefore define *two* gravitational masses—a field-creating mass and a field-responding mass. The former is proportional to the inertial mass and the latter has no particular value; the response of a body to a gravitational field is quite independent of it. Strangely enough, however, it is only the latter that is called 'gravitational mass'; the former is ignored.

All this raises great conceptual difficulties. It is quite natural to suppose, when a body creates a certain condition in the space around it, that it possesses some property by which it does so. An electric field is created around a charged body, so we accredit the body with the possession of an electric charge, to which we assign a magnitude indicated by the strength of the field. Similarly, when a gravitational field is created around a massive body, we should accredit the body with the possession of a property whose magnitude is indicated by the strength of the field. But we do not. We bestow the name 'gravitational mass' on the supposed property by which a body *responds* to a field, although the response is the same whatever the measure of the property. (Note, however, that this applies only to the *initial* acceleration. The force between two bodies increases as they approach one another, with corresponding increases in the accelerations, and the rates of increase depend on the inertial masses of the bodies). This is made quite clear by Einstein's equations in the passage quoted above: the 'gravitational mass' is the property corresponding to a body's behaviour in an independently created field, not the property by which a body creates a field.

This anomaly in the choice of a gravitation-related property for a body is usually overlooked because the example usually chosen to illustrate this assumed mysterious identity of inertial and gravitational mass is a very special one; it is the fact that a 1 lb. and a 1 oz. weight, say, fall similarly towards the Earth. But that is only approximately so, even with regard to the initial accelerations, because the Earth is so much more massive than the weights. The Earth and the Moon do *not* fall similarly towards the 1 lb. weight—a fact nowadays popularly expressed in the inverse form that gravity is weaker on the Moon than on the

Earth. Gravitation, on Newton's theory and in fact, is a *mutual* attraction, and its division into two parts, depending respectively on independent properties of the two bodies concerned, necessarily introduces difficulties which, if not insuperable, have not yet been surmounted.

II

Let us turn now to the general question: is nature best described in field or in particle terms? I am not concerned here with the actual state of physics at any particular time—i.e. with the question whether existing knowledge makes one or the other form of expression more suitable for immediate progress. The historical facts bearing on that question are set out and examined in some detail in Dr Mary B. Hesse's book, *Forces and Fields* (Nelson, 1961). What I shall examine is the *a priori* character of the two types of theory and the question whether that affords any ground for supposing that one rather than the other will finally prevail. I shall, moreover, confine the discussion to mechanics, leaving open the question whether the possibility or otherwise of a unified field (or particle) theory, comprising all physical phenomena, might modify the conclusions reached from a consideration of mechanics. That is not because such a question is unimportant but because it is premature. Our knowledge of the relations between the major departments of science—mechanics, electromagnetism, thermodynamics, . . . is as yet too slight to give any value to speculations concerning the character of their ultimate unification. Mechanics, however, in so far as it can be considered in isolation, is covered pretty comprehensively by both Newton's and Einstein's theories, so an examination of the prospects within that limited region might not be profitless.

A pure field theory is one in which only those parameters appear that describe the state of a field. What we usually call a particle or an extended body would be distinguished, on such a theory, from what we call the surrounding empty space only by the fact that those parameters would have different values at the corresponding places in the field; there would be no qualitative difference, only a quantitative one.

A pure particle theory, on the other hand (the word *particle* is used for convenience to indicate any isolated body, not necessarily one of infinitesimal dimensions), is one in which only those parameters appear that can be thought of as properties of bodies—for example, mass, shape, hardness, . . . would be possible, but not the distance between two bodies, for that is not a property of a body. (We must, however, apparently allow time as a possible parameter in each type of theory, as something more fundamental than either: no description of nature can be conceived from which time is excluded.)

Neither a pure field nor a pure particle theory is *a priori* impossible; on the other hand, neither can be accorded any appreciable probability of attainment. In Einstein's theory, for example, the field in which the Earth moves cannot be described without the postulation—arbitrary so far as the theory is concerned—that the Sun is where it is and what it is. In an extended discussion of the possibility of a pure field theory, Einstein and Infeld,[1] without attempting to disguise their view that such a consummation is devoutly to be wished, nevertheless acknowledge that it is not within sight. They write (pp. 257-8):

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[1] A. Einstein and L. Infeld, *The Evolution of Physics* (Cambridge, 1938).

> 'Could we not reject the concept of matter and build a pure field physics? . . . Our ultimate problem would be to modify our field laws in such a way that they would not break down for regions in which the energy is enormously concentrated.
>
> 'But we have not so far succeeded in fulfilling this programme convincingly and consistently. The decision, as to whether it is possible to carry it out, belongs to the future. At present we must still assume in all our actual theoretical constructions two realities: field and matter.'

It is impossible to dissent from this conclusion.

But neither is a pure particle theory necessarily impossible. It would include nothing corresponding to what we now call empty space, but then there are no phenomena in empty space, so that omission need not prohibit the full description of everything observed. What we now call 'action at a distance' would involve no conceptual difficulties—at least, none but such as might arise from our inability to rid our thoughts of the field concept of space. It is the action, not the distance, that is our datum, and if we can describe all actions in terms of the properties of particles alone, we can keep within our programme and leave nothing observable out.

All existing theories, however, include both field and particle conceptions, and when we call Einstein's gravitational theory a field theory, and Newton's a particle theory, we are merely expressing their dominant features: Einstein's is more a field than a particle theory, and Newton's more a particle than a field theory. Is there any reason to expect that anything other than a combination of the two will be finally successful?

I do not think there is. It is, after all, only a matter of aesthetics that leads us to seek purity in this matter, and taste is notoriously variable. The only really pertinent consideration is whether phenomena themselves, which are the raw material of all theories, are homogeneous, and in fact they are not. Some concern only individual bodies, others are essentially *relations* between bodies. The shape of a body can be described without reference to anything else; its motion cannot. It is true that, in the case of motion, the other something may be imaginary, but that is not so with regard to the Doppler effect, which is inseparable from relative motion. That has nothing to do with the motion of either body; it is determined wholly by the relation between their motions. Since, therefore, we have to deal with properties of both single bodies, for which field concepts are superfluous, and groups of bodies, for which particle concepts are inadequate, it would seem that a theory competent to cover both would have to include both sets of concepts.

If that is so, the goal of Einstein and Infeld's aspiration is El Dorado; and indeed, without considering whether *any* pure field theory is possible, we can see that the particular field theory known as general relativity cannot possibly be developed to cover the whole of mechanical phenomena, for it has, at the beginning, one self-imposed limitation that puts this out of the question—namely, that it supposes that everything that occurs can be analysed into *point-instant events*. Its fundamental postulate is that everything mechanical in nature can be expressed as a relation between events which are completely characterised by the instantaneous values of four co-ordinates, x, y, z, t. But in fact there are events for which such an analysis is impossible. As a single example, suppose a ball is thrown upwards and returns. There is certainly a single event at which its distance from the ground is a maximum, but that event is not expressible in current relativity theory. It can only be described as either the event of the ball attaining its greatest height above the ground, or the ground reaching its greatest depth below the ball, and these are two different events, occurring in different places and—except for a particular co-ordinate system—at different times. Which of these events is the actual event of maximum separation is quite indeterminable, and that is because the theory has no means of describing events that are essentially not point-instants.[1]

This, of course, does not mean that a (predominantly) field theory is impossible, but simply that such a theory must include parameters relating to unanalysed *finite regions* of space (or space-time) and not only point-instants. For that reason, Einstein's failure to achieve a satisfactory unified field theory—a single theory including his own gravitational theory and the Maxwell-Lorentz electromagnetic theory—is not necessarily an indication that a unified field theory is impossible. The failure might be due to this defect in the gravitational theory assumed and possibly a defect in the Maxwell-Lorentz theory also.[2]

Nevertheless, if we ask ourselves what type of theory is most likely to produce a completely satisfactory account of mechanics, it is hard to avoid deciding for a predominantly particle theory. As has been said, the difference between the existing representatives of the two types—Newtonian and Einsteinian—with respect to their agreement with phenomena is too slight and doubtful to have much significance in the long view. But the completely natural combination of the particle concept of mass and the field concept of distance that characterises Newton's theory contrasts so favourably with the artificial association of Einstein's space-time concept with the existence of discrete matter as to make its potentialities for successful development appear much greater. Newton's theory does not compel reference to co-ordinate systems, though it can, if desired, use them. A conception of space which is independent of time gives the distance between two masses an absolute character in itself; it is not necessary to assume an infinite variety of values and regard as significant only what is compatible with all of them. Einstein's theory, on the other hand, *must* use co-ordinate systems, since its space-time field is not otherwise applicable to observations, and while it avoids many of the dangers which they bring with them by insisting that only those functions of co-ordinates that are independent of the particular system employed shall correspond to measurements, it is not easy to see how the formulae of transformation from one system to another, which are necessary to ensure that this independence exists, can be extended to cover the finite regions of the field, as well as points, which phenomena require to be acknowledged.

The justification for speculations such as these, however, is not that they can provide a basis for a successful bet, but that they help to free current research from slavery to fleeting appearances. We are at an arbitrary point in a continuously changing process; at earlier epochs the prevailing conceptions have invariably had to give place to others, often those thought to have been finally disposed of.

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[1] For other examples of events not analysable into point-instants, see *Observatory*, 85 (1965) 262.

[2] See this *Journal*, 15 (1964), 41, for evidence of the latter possibility.

There is no reason to suppose that the present epoch is exceptional in having reached finality in any respect. Research in gravitation theory is now everywhere restricted to field concepts, and the possibility of a development of Newton's particle theory is ignored on the ground, not of its intrinsic nature, but of its poor capacity for distortion into the field form, which is misinterpreted as an intrinsic defect. It is necessary to look at the historical process from a detached point of view, from which the present moment is undistinguished from others, in order to approach the future with the best prospect of successful advance.

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THE CONFUSION CONCERNING UNIVERSAL FORCES

There is an equivocation on the term 'universal forces' which originated in Reichenbach's *Philosophy of Space and Time* and has resulted both in an inconsistency in the use of the term in the literature and perpetuation of a basic misunderstanding of the fundamental statement of general relativity.

Universal forces 'affect all materials in the same way' (Reichenbach, 1958, p. 13). The question is whether this means that they produce the same deformations on all bodies, or whether they impart the same acceleration to all spherically symmetric mass points. Reichenbach does not distinguish between these two very different senses, and slips from one to the other in his discussion. At first he discusses universally deforming forces, under the influence of which 'not only measuring rods . . . but all objects . . . are affected in the same way' (1958, p. 12) He contrasts these with differential forces, forces which produce substance-specific deformations. However later in his discussion he says

> We have learned in §6 about the difference between *universal* and *differential* forces . . . We find that gravitation is a universal force. It does indeed affect all bodies in the same manner. This is the physical significance of the equality of gravitational and inertial mass (1958, p. 256).

The physical significance of the equality of gravitational and inertial mass requires that gravity be a universally accelerating force *not* a universally deforming force. Furthermore whereas this equality is fundamental to the general theory of relativity, consideration of universally deforming forces and transportation of rigid rods is irrelevant to Einstein's general theory; nothing could be transported around a space-like hypersurface since the events composing such a hypersurface are causally independent.

Among philosophers the term universal force is generally taken to mean a universal force in Reichenbach's first sense, a universally deforming 'force'. Thus Hilary Putman (1963, p. 284) and Adolf Grünbaum (1963, p. 92)[1] have objected to Reichenbach's classification of gravity as a universal rather than a differential force since gravity deforms different bodies differently.

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[1] Note that the distinction made above is not Grünbaum's distinction between metaphorical and literal universal forces.

On the other hand when physicists employ the term it is to mean a universally accelerating force (Adler *et. al.*, 1965, p. 3). One must assume that this is the sense which H. P. Robertson has in mind when he contrasts the general theory of relativity with an incorporation of the effects of temperature into the geometry, since he mentions the proportionality of inertial and gravitational mass. However he says:

> In what respect, then, does the general theory of relativity differ in principle from this geometrical theory of a hot plate? The answer is: in its universality; the force of gravitation which it comprehends in the geometrical structure acts equally on all matter (Robertson, 1949, pp. 328-9).

This statement obscures the distinction between the two senses of universal force making it appear that gravity differs from temperature in producing the same effects on all substance. One would then expect that gravity produces universal deformations. However this is not the case. Whereas the effects of temperature only require reference to space, the accelerations due to gravity essentially require reference to *space-time*. It is the so-called geometry of space-time, not the geometry of space, which incorporates the effects of gravity.

It seems that this last point may have been clear to Reichenbach himself, for althought he often speaks of gravity as a universally deforming 'force' (1958, pp. 26, 36), his principal discussion of gravity falls under his treatment of space-time while his discussion of rigid rods is found in his section on space. The difficulty remains that the analysis of universal deforming 'forces' affecting space does not carry over to consideration of universally accelerating forces affecting space-time. Considering space-time there is *no* analogue to self-identical rods which may be transported or which may undergo deformations. Furthermore the fundamental space-time invariant ds^2 fails to have the properties of a distance function, that is it may take on negative values or assign a value of zero to a pair of distinct points. Moreover the value of the ds^2 assigned to a pair of space-time points is not in general directly measurable, but must be calculated from measurements of spatial separation and temporal separation. The effect of gravity upon bodies neither is, nor is the analogue of, universal deformation.

The account in Einstein's own popular exposition of relativity may also have fostered this confusion. There Einstein gives the example of a reference system K′ on a disk which is made to rotate uniformly with respect to a Galilean reference frame K. Since the edge of the disk is moving faster than the parts near the centre, measuring rods placed on the disk tangential to rotation will undergo relativistic contraction from the standpoint of K, which increases with distance from the centre of the disk. The result is that with respect to K the ratio of the circumference of the disk to its diameter will be greater than π (Einstein, 1961, p. 82). Unfortunately Einstein goes on to say that this situation corresponds to that which would be obtained if temperature, instead of producing substance-specific distortions, influenced all bodies in the same way (1961, pp. 85-86). However the correspondence between the two situations is very weak even in this special case of a uniformly rotating disk, as a little attention to the latter will show. Rods placed radially undergo no contraction (Einstein, 1961, p. 81), but the force which is a gravitational force for K′ and an inertial force for K, acts in this radial direction, *not* in the tangential direction. Different deformations of bodies will result in the radial direction. Consider the various distortions produced on different objects in a centrifuge.

The above considerations serve to show that a discussion of universally deforming 'forces' has no bearing on the incorporation of the gravitational force into the space-time structure predicted by the general theory of relativity, and that alternatives to this theory do not require postulation of universally deforming 'forces'.

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Reviews

Carl G. Hempel: *Aspects of Scientific Explanation; And other Essays in the Philosophy of Science* (The Free Press, New York, and Collier-Macmillan, Ltd., London, 1965, 95*s*.) Pp. ix+505.

If it is true that positivist and operationist philosophy of science have been almost universally abandoned and replaced by some form of deductivism, it is also true that one of the chief agents of this transformation during the last twenty-five years has been Professor Carl Hempel. Four of his classic papers reprinted in this volume are 'Studies in the Logic of Confirmation' (1945), 'Studies in the Logic of Explanation' (1948), 'Problems and Changes in the Empiricist Criterion of Meaning' (1950), and 'The Theoretician's Dilemma' (1958). In these he unerringly exposed the fundamental logical weaknesses of most forms of positivism, and articulated for a new generation of philosophers of science new criteria for the justification of explanatory science. In seeking to apply his deductive-nomological and inductive-statistical models of explanation over the whole range of the sciences, and also into the study of history, he has raised a host of issues which continue to provoke lively discussion in current literature. In this volume, in addition to convenient reprints of many of his papers, we have Hempel's own response to some of the subsequent debates, in the form of postscripts, and in a new systematic essay entitled 'Aspects of Scientific Explanation', which makes up one third of the book.

Hempel's approach to the philosophy of science can best be characterised in terms of *explications*. Given a concept such as 'justification', 'confirmation', 'explanation', 'meaning', the problem is to state criteria of adequacy for its application in scientific contexts. It is misleading to claim, as some of Hempel's critics have done, that such an approach is unduly formal. It is more correctly described as pre-formal, in that it seeks the conditions under which any formal theory, for example of confirmation, will adequately represent what we informally require of a good piece of scientific inference. A search for such conditions by no means reduces philosophical questions to logical questions, it rather uncovers the philosophical issues lurking in our intuitive assumptions about science, and subjects them to critical examination. The questions raised in this form include the nature and justification of explanatory theories, statistical explanation, criteria of empirical meaning, causality, explanation in history, and functional analysis. Of all these the most pervasive and perhaps the most important concern explanatory theories, and I shall pick some topics from this area for particular comment in this review.

Hempel's 'Studies in the Logic of Confirmation' is best remembered for his discovery of the so-called paradoxes of confirmation. A re-reading of this article, however, reminds one that the paradoxes were in a sense only an incidental embarrassment to the main aim of the paper, which was the attempt to spell out criteria for an explication of confirmation. It is worth pursuing the discussion on these criteria a little way, in order to indicate the potential richness of the method of explication itself, and also because subsequent discussion of the paradoxes has all-but-silenced more general and ultimately more important questions raised in Hempel's paper.

Most discussions of confirmation have restricted themselves to simple inductive generalisations of the 'All ravens are black' type, but deductive theories in general are not only much more complex in logical form than this, but are also used in making predictive inferences which must also be regarded as inductive, and which should therefore be capable of being explicated by a successful confirmation theory. Consider the following familiar theoretical inference: Newton's theory is proposed on the basis of a set of experimental laws which are entailed by the theory, for example the law of falling bodies, Kepler's laws, the orbits of the comets, and so on. Further as yet unobserved consequences are then drawn from the theory, for example the laws of the tides, and these are predicted with confidence on the basis of the confirmation of Newton's theory by its already observed consequences. Indeed, sometimes confidence in the consequences of he theory is so strong that it enables *corrections* to be made even to laws which have already been accepted, for example Newton's theory corrected Kepler's third law as accepted on the observed evidence of the periodic times of the planets. Among the criteria of adequacy for confirmation discussed by Hempel are two which seem to be required for explication of this kind of theoretical inference (pp. 31-32):

(1) *Converse consequence condition:* If H entails E, then E confirms H.

(2) *Special consequence condition:* If E confirms H, E confirms every logical consequence of H.

Under these conditions, the laws entailed by Newton's theory confirm Newton's theory by (1), and hence all other consequences of Newton's theory by (2). Unfortunately, as Hempel notices, serious trouble arises if *both* these conditions are adopted, in fact together they entail that any evidence will confirm any statement S whatever by mere construction of a suitable H, for example by conjoining E and S. For this reason, Hempel originally rejected (1) and retained (2), thus demanding a stronger relation between hypothesis and evidence than mere entailment if the evidence is to confirm the hypothesis. But it is a further consequence of accepting (2) and rejecting (1) that the confirmation function cannot be a probability function, for in a probabilistic confirmation theory, (1) is always satisfied for empirical E. Furthermore, (2) is then not in general satisfied, for if E_1 is to confirm E_2 probabilistically there must be a stronger relation between E_1 and E_2 than their joint deducibility from some hypothesis H. These difficulties were first demonstrated by Carnap in *Logical Foundations of Probability* (pp. 471 ff), in what is still the best discussion of the general implications of Hempel's paper. Hempel now accepts that condition (2) rather than (1) should be abandoned, and agrees that a probabilistic confirmation function seems to provide a better explication of confirmation than a function satisfying his original criteria (p. 50).

It is noteworthy that even in the original paper Hempel suspected that an adequate confirmation theory would involve more than the deductive relations on which his conditions depend:

> 'Unquestionably scientific hypotheses do have a predictive function; but the way in which they perform this function, the manner in which they establish logical connections between observation reports, is logically more complex than a deductive inference' (p. 28).

Little progress has subsequently been made towards examination of these 'logically more complex' inferences, which would have to rely upon logical relations between E_1 and E_2 which are stronger than their mere deducibility from some hypothesis H. In the absence of such investigations it cannot be said that the deductive account of theories has succeeded in coming to grips with some of the most important patterns of theoretical inference.[1]

The inadequacy thus demonstrated in current confirmation theories is also relevant to Hempel's paper 'Studies in the Logic of Explanation'. Here, as is well known, Hempel at first required as an empirical condition of adequacy for an explanation that the sentences constituting the explanans must be *true*. This is so strong a requirement that no actual candidate for an explanation could be known to satisfy it, and Hempel therefore weakened the requirement in various ways. In the new essay in this volume (p. 338), he states an alternative as follows: An explanation may be qualified as '*more or less strongly supported or confirmed* by a given body of evidence according as its explanans is more or less strongly confirmed by the given evidence'. Hempel's instincts are surely right in holding that some such requirement should be added to the deducibility conditions of adequacy of an explanation. For if there were no such empirical criterion of confirmation, there would be no way of eliminating a large number of deductive systems satisfying the deducibility conditions, but so arbitrary as not to be regarded as even potentially explanatory. If the confirmation criterion is allowed to stand, however, the demonstrable absence of a satisfactory confirmation theory affects not only the question of inductive justification of theories, but also the nature of explanation itself.

The deductive model of explanation has recently come under strong attack for other reasons, and it is a pity that Hempel gives space only in a footnote to a reply to Feyerabend's criticisms of deductivism (p. 347). Feyerabend claims that deductivism demands 'that all successful theories in a given domain must be mutually consistent', and Hempel has no difficulty in showing that his own deductive-nomological criteria of explanation make no such demand, because the same explanandum may be explained in this sense by mutually incompatible theories. But Feyerabend's claim is only part of a much more interesting thesis according to which *it does not make sense to say* that the same explanandum is explained by mutually incompatible theories, because the explanandum in the context of one theory will be so permeated by the conceptual framework of that theory that it cannot be 'the same' as an explanandum explained by an incompatible theory with its different conceptual framework. On this thesis Hempel does not comment in the present volume, and one hopes that he will do so in subsequent publications.

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[1] I have made some suggestions towards explication of these inferences in 'Consilience of Inductions', forthcoming in *The Problem of Inductive Logic*, ed I. Lakatos, Amsterdam, 1967.

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Hempel's most recent thoughts on the relation of theory and observation would be especially valuable, because there are already in this volume hints that he has never wholeheartedly accepted the dualism presupposed by most other deductivist writers. In a comparatively early paper he suggested that with regard to theoretical systems 'we will have to recognise . . . that cognitive significance in a system is a matter of degree' (p. 117), and explains that the degree depends on the proportion of 'theoretical' to 'observational' terms in the system and on its confirmation, its formal clarity, simplicity, etc. Similar remarks are made about the relative character of the experimental significance of so-called 'operational' terms (p. 132). 'Degrees of cognitive significance' is perhaps not the happiest expression in this context, but at least it suggests that the distinction of theory and observation is only a first approximation which requires further scrutiny. Echoes of a more orthodox dualism are heard, however, in Hempel's banishment of the use of 'analogy' in theorising to its *pragmatic* aspects (p. 422), and in his discussion of functional analysis, where he notes several examples of circular and imprecise use of key terms such as 'need', 'function', 'survival', and quotes with approval a dictum that 'precision is an integral element of the criterion of *testability*' (p. 323). While it is undoubtedly true that *some* uses of such terms may be unacceptably vague or circular, the admission that there are *degrees of* empirical testability, and that empirical significance depends on factors other than 'operational' observability, suggests that there is something irreducibly imprecise and even circular about the use of *all* observation terms—or, in Wittgenstein's phrase, that successive correct uses of a term have at best 'family resemblance', and are not determined by precise conditions. Further investigation is urgently needed in this area, and if it leads, as it almost certainly will, to a general relativisation of the too sharp distinctions between the theoretical and the observational which have been made in recent orthodoxy, Hempel will have proved once again to have been a pioneer.

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Carl G. Hempel: *Philosophy of Natural Science*. Englewood Cliffs, N.J., (Prentice-Hall), 1966, 16*s*. Pp. xii+116.

This volume is one of a series called *Foundations of Philosophy*, and edited by Elizabeth and Monroe Beardsley. It is intended as an introduction to some of the central topics in contemporary philosophy of natural science, for use in college classes. The author begins by taking Semmelweis's work in childbed fever as a case history of scientific investigation in which a succession of hypotheses are tested in turn. Various different types of test, and the conclusions to be drawn from them, are then discussed, with further illustrations drawn from the history of science. There is a brief discussion of the problem of simplicity, and a very brief one of degree of confirmation. Explanation is then discussed at some length, and some attention is given to the differences between universal laws and accidental generalisations and between nomological and statistical explanation. The structure of theories and the status of theoretical entities are examined next, and some criticisms of operationalism are offered. A final few pages are devoted to the mechanism—vitalism issue, and to discussing what is involved in 'reducing' one branch of science to another.

Hempel's own contributions to the philosophy of science are well known. There is little doubt that he is to be numbered among the most outstanding and important half dozen thinkers of his generation in this subject. It would therefore be quite pointless and unfair to comment on the substance of what he says in this book about induction, explanation, and theory-construction. He has said nearly all of it elsewhere in a form that is more suited to professional criticism. It should suffice to remark here that every sentence, every paragraph and every chapter is written with the clarity and incisiveness that has always characterised Hempel's work, and that a better elementary introduction to Hempel's own views in the philosophy of science could scarcely be written.

Nevertheless, the reviewer is very doubtful whether this book is at all a good introduction to the subject for college students. Suppose you were confronted with an introductory textbook of physics, say, or physiology, that summarised very clearly and succinctly, and without too much technical jargon, what the author took to be established and incontrovertible in his field, but said nothing at all about methodology—i.e. nothing about the conduct of experiments, measurement problems, statistical technique, and so on. Would you not be inclined to say that this was a good piece of journalism, but somehow missed the guts of the subject? It would seem to treat the subject more as so-and-so much doctrine to be communicated, than as a living, continuing enquiry. It might help students to get to know the conclusions to which physicists or physiologists have come, but it would give them little help to become physicists or physiologists themselves.

Hempel's book seems to suffer from an analogous defect. Just because it is so good an introduction to the substance of Hempel's own philosophical analyses, it has almost nothing to say about the methodology of philosophy of science. No doubt there was a time (roughly 1930-60) when philosophers thought, spoke and wrote too much about the nature of philosophical analysis. But perhaps they do so too little now. A student unfamiliar with the recent history of the subject might read Hempel's book and still be quite at a loss to know how philosophers of science operate. Do they set out to describe what actual scientists do? If so, what motivates their selection of some features to describe and their neglect of others? What distinguishes the philosophy of science from its history or from the study of its financial, organisational and communicational problems? Or do philosophers set out instead to characterise what ideal scientists do? If so, how are their characterisations supported, and in particular how can it be shown that a single set of ideals operates or should operate? Has oral discussion an important role to play in philosophising about science, as has sometimes been claimed in other branches of philosophy, or are there special reasons why it has less value in this field? Is there anything at all to be said for the view that at its heart philosophy of science, like all other philosophy, is activity rather than doctrine and seeks to dissolve intellectual puzzlement rather than to propound theoretical solutions of theoretical problems? These seem to the reviewer to be some of the issues that an introduction to the philosophy of science needs to tackle, alongside whatever it has to say about matters of substance within the subject. But for those who are prepared to dispense with all but the latter Hempel's book is excellent.

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Morton White: *Foundations of Historical Knowledge* (New York & London, Harper & Row, 1965, $6.95.) Pp. ix + 294.

The history of the philosophy and methodology of history in the last twenty-four years, it might be said, is a series of footnotes to Hempel. The literature in the field has been dominated by criticisms, revisions and defences of the so-called covering-law theory of explanation, but in the last year or so, writers have begun to discover some fresh issues. A large part of Morton White's book remains in the old tradition—he defends a variant of the covering-law analysis of causal explanation—but he takes the actual practice of historians, and the methodological problems which bother historians, more seriously than have many philosophers, and discusses a broader range of issues than do most writers in the explanation controversy. One chapter is devoted to a discussion of the structure of narrative and the various criteria for choosing among factually correct narrative accounts. Another contains an illuminating treatment of the criteria for selecting *the* cause, or the most fundamental cause, from among the contributary causes. The book concludes with a discussion of free-will and determinism. In this review, I want to consider, first, some doubts that I have about the theory of "existential regularism", White's variant of the covering-law analysis; second, some problems with his criticism of the theory of rational explanation, and third, some virtues of the section on causal interpretation.

It is obvious that the alleged explanations offered by historians are not full-fledged deductive arguments. Anyone who wishes to use the covering-law model as a reconstruction of these arguments must give some account of the deviation from the standard. Hempel's thesis is that an explanatory statement given by an historian is usually a *sketch* of an explanatory argument. In so far as the sketch makes a claim, it is that the future course of inquiry will turn up a full dress explanatory argument which makes essential use of the antecedent conditions that are mentioned. White argues that a statement of the form 'A is a contributory cause of C' is not an ellipsis for or a sketch of a deductive explanatory argument, but is equivalent to the claim that *there exists* an explanatory argument of the covering-law type. It is possible to defend such statements, he argues, without being able to produce the argument itself. The principal problem with this analysis is that it involves quantification over laws; a singular explanatory statement entails that *there exists* a law, or laws, of a certain type, even though such laws may not be known, now or ever. For the analysis to have any content, it is crucial that law-like and non-law-like universal generalisations be distinguished.

Hempel can put aside the problem of giving a *general characterisation* of the distinction between these two kinds of universal statements as a separate problem. As long as we can tell the difference in particular cases, we can understand and apply Hempel's thesis about explanations and explanation sketches. White's thesis, however, depends more heavily on a general account of the distinction: we must know *in general* what a law is in order to understand the claim that *there exists* one. Moreover, the distinction must be independent of the pragmatic factor such as entrenchment that Goodman and others have used to help mark the difference; otherwise, the existence of a law is not independent of its discovery and use. But it is not clear that the existence of laws can be independent of their discovery and use, as White's thesis requires.

However *this* issue is resolved, there is one way of marking the distinction that clearly will not do. White says 'if a statement is equivalent to a conjunction of a finite number of singular statements, it will not . . . be a law' (69-70, cf. 32). But *no* universal generalisation, law or non-law, is *logically* equivalent to any finite conjunction of singular statements, and *both* laws and non-laws will be *materially* equivalent to such a finite conjunction. The distinction between laws and other universal statements cannot be drawn so easily in terms of the logical force of the generalisations.

In his discussion of reasons and causes, White attacks a thesis according to which 'a historian can understand an action if and only if he regards it as a reasonable action, and he can regard it as reasonable if and only if it falls under some principle or normative rule which he, the historian, accepts' (183). I agree that this thesis is indefensible, but I know of no one who holds it. It should be made clear that Dray, whose name is most closely associated with the theory of rational explanation, explicitly states both that (1), explanation in terms of reasons is only *one* kind of explanation; covering-law explanations are appropriate in the case of irrational actions, or other events, and (2), the historian need not accept the practical principles he uses to give a rational explanation of an action.

White points out, in his argument against the thesis, that the premisses, 'A believes p' and 'p entails q' are not logically sufficient for the conclusion, 'A believes q' (187-8). This is of course true, but it shows only that the model of rational explanation of beliefs or actions is not the same as the covering-law model, which is Dray's point. Dray's claim is not that these 'premisses' provide a causal or covering-law explanation of the 'conclusion', but that we do not *need* a *causal* explanation for valid inferences, or for actions which follow from the beliefs and principles of the agent.

After attacking the extreme version of the theory of rational explanation, White grants that explanations in terms of reasons are sometimes legitimate, and then develops a somewhat obscure distinction between causal and non-causal rational explanations.

In the chapter on causal interpretation, White argues that the basic criterion for selecting *the* cause from among the set of contributory causes and conditions is the statistical abnormality of the cause. The application of this criterion, he notes, is dependent to some extent on the category in which the event to be explained is placed, for the purposes of the explanation, which in turn is dependent to some extent on the interests of the historian. This discussion is particularly interesting from the point of view of the controversy over *historical relativism*. A generation or so ago it was fashionable among some groups of American historians to hold that written history is relative to the present interests and presuppositions of the historian. The thesis is ambiguous, and the arguments for it were largely confused; no attempt was made to analyse the relationship between 'objective facts' and 'relative' interpretative judgments, and the thesis was used by some of its defenders as an excuse to be cavalier about the facts. White's more philosophically sophisticated treatment of the way in which the historian's interests may affect his evaluation of historical claims gives coherent content to the idea behind the relativist position. His analysis of causal interpretation explains *how* the interests and value judgments of historians have their effect, and how the selection of the fundamental cause may be not capricious, and yet not unambiguously dictated by the facts.

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Jonathan Bennett: *Rationality.* (London, Routledge & Kegan Paul, 1964, 14*s.*) Pp. vii+123.

This addition to the Studies in Philosophical Psychology presents 'the results of an extremely ambitious conceptual enquiry into the nature of rationality' (p. 1). It would be most gratifying to all philosophers working in this field to find that the complex concept of rationality, central to so many different branches of philosophy, can be adequately analysed in a monograph of this brevity. Though Mr Bennett writes succinctly and concisely, his analysis is unsatisfactory and often confused.

The problem Bennett poses is to discover what it is that marks off human beings, with respect to intellectual capacity, from all other species (p. 5). Humans are rational, animals are not. But what is it to be rational? Bennett's felicitous procedure is to start with a form of behaviour that is intuitively non-rational, and to discover what features must be added to it for it to be rational. Rationality, he emphasises, is a property of behaviour, and not a relation between behaviour and thought. What are the criteria for ascribing rationality to an agent's conduct? He begins by describing the dance of bees, analysed in detail by Von Frisch. Bees do not have a language, since the four features necessary for identifying behaviour as linguistic are absent. There must be, Bennett claims, rules relating signs with the world, i.e. a semantic dimension, a behavioural element on the basis of which we can say that the agents understand each other, a complexity enabling new sayings, i.e. a syntactical element, and finally the behaviour must be symbolic, i.e. there must be a conventional association between behaviour and fact. The latter point seems to be a reiteration of the first. Apian dances are in fact mere regularities without reasons. An apian myth is ingeniously developed, in the course of which the following elements are added to the Von Frisch description. (1) The bees' conduct becomes *prima facie* rule-governed if they show an awareness of breaches of rules *as* breaches of rules by performing a denial dance when a rule is broken and seen to be broken. This, however, is insufficient. (2) The bees perform a denial if they are in a dance-indicated place which does not contain food-traces. There is thus a *prima-facie* case for thinking that the bees have the notion of evidence. This endows them with intelligence, but not rationality. Intelligence is possessed by an animal whose patterns of behaviour '(a) involve the organisation and control of complex data, and (b) are suitably modified when they no longer prove appropriate in the light of the animals needs' (p. 36). Intelligence and symbolising capacity are necessary for possession of a language, but not sufficient. The intelligence must be exercised in certain ways. (3) The bees are now endowed with the capacity for making statements in two distinct tenses concerning two distinct subjects of discourse, and means for stating reasons for affirmation and denial. But how can the observer distinguish whether a bee's dance signifies 'p for the reason that q', rather than 'p and q'? This is only possible if (4) the bees can deny not merely the primary assertion that p, but also that q is a reason for p. But it must also be possible to deny the denial that q is a reason for p. For this the bees must (5) be able to make and assess statements about the relatively remote past, and to make general statements. Dated and universal statements lie at the heart of rationality (p. 80) 'The idea of rationality is that of the ability, given certain present and particular data, to unite or relate them with other data in certain appropriate ways' (p. 85). Linguistic capacity does not entail rationality. A sheepdog's response to a shepherd's whistle comes close to understanding of a primitive language. But the expression of data and universal judgments is both necessary and sufficient for rationality. Bennett tries to establish this in chaper 11 in which a creature called 'Describer' is invented who possesses a language and renders true descriptions of its immediate environment, but cannot make dated and universal statements. He concludes 'The word "rational", however, is not my central concern: it will suffice if we can agree that there is a great gulf between the sorts of competence which are expressed in dated and universal statements and the sorts which are not, and that real dogs and the Describer lie on one side of this gulf while humans and my final bees lie on the other' (pp. 97-98).

The following critical comments come to mind. (1) Bennett begins by claiming that rationality is primarily a property of behaviour. However we are given no criteria for ascribing rationality to behaviour, but merely told that only a being that can make dated and universal statements is rational. (2) It is not clear that this is sufficient for possession of rationality since (*a*) It commits us to viewing an appropriately programmed computer as rational, and it is not clear that this is correct. (*b*) A creature that has this capacity may lack the ability to pursue rational goals. A being that preferred the scratching of its little finger to the destruction of the world would, *pace* Hume, be irrational, even though it could make dated and universal statements. (3) It is not clear that it is necessary for rationality, since philosophers have always considered deductive reasoning, as in mathematics or logic, as the paradigm of rational thought, although it involves no dated statements. (4) The imprecise definition of rationality on page 85 does not demonstrate that intelligence, as defined on page 36, is not sufficient for rationality so defined. (5) The cursory analysis of language is faulty. It systematically confuses the necessary conditions for having a language with what is necessary for an observer to identify an action as an utterance. It is necessary if an action is an utterance that it be done for reasons, standardly to inform others (p. 44). This presumes, wrongly I submit, that it is logically impossible for Man Friday, alone on his island, to have a language, and hence to be rational. (6) No one would quarrel with Bennett's conclusion on pages 97-98 that a tensed language with universal quantifiers constitutes an immense advance over a language which lacks these features, and gives great conceptual potentialities. But is this rationality? Bennett states correctly that we need not be interested in the word 'rational', but we are concerned with the concept of rationality. Conceptual analysis or explication is aided by clarification of the explicandum—this is never done. Rationality is predicated of actions, agents, goals, feelings, beliefs, conclusions, etc. Bennett purports to be analysing the first, attempts unsuccessfully to analyse the second, and disregards the others. The analysis of rationality requires greater care and more thought than this.

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Friedrich Waismann: *The Principles of Linguistic Philosophy*. Edited by R. Harré (London, Macmillan, 1965, 35*s*.) Pp. xii+422.

The first version of this book was completed before the Second World War, but Waismann withdrew it on the eve of publication, on Wittgenstein's advice or command, so rumour has it. According to the editor, Waismann reworked it constantly until his death in 1959; and what we have here is, we are told, 'a continuous exposition, as far as possible using only what seems to have been the latest of the modifications and additions which he had written'.

The book is in two parts: the first, called 'The Transition from the Classical to the Linguistic View of Philosophy', is a fairly general survey of what Waismann takes to have happened in philosophy during the present century, with examples from such problems as the trustworthiness of memory, the possibility of sharing experiences, truth, and *a priori* knowledge; and an account of why philosophical problems arise when they do. The second and much the longer part, called 'Elements of a Philosophical Grammar', is almost entirely concerned with matters in the theory of meaning, and includes a lengthy discussion of rules, a surprisingly brief account of meaning as use, where no attempt is made to clear up the numerous misunderstandings to which that view has given rise, a discussion of ostensive definition, and of the relationships between meaning and verification; and also a great deal about the nature of propositions, involving analysis of parts of the *Tractatus*, especially the Picture Theory, of which very familiar criticisms are advanced. The book ends with two chapters on the propositional calculus, the relevance of which to its central themes is not obvious, and a chapter ostensibly on the logic of questions, which is in fact mainly about different kinds of search, especially that which mathematicians conduct for proofs.

The result is a disappointment to anyone who was hoping for fresh arguments about or insights into the problems with which Waismann is concerned. To a large extent these are the same as those with which the *Philosophical Investigations* deals, and so are his views on them. But the impression it makes is very different. Apart from a number of aphorisms such as '(Philosophy) overthrows idols, and it is the importance of these idols which makes philosophy important' (p. 78), and a few stylistic similarities, the total effect is at the opposite pole to that of the

Investigations. On every page of that book one feels the presence of an extraordinarily penetrating and original mind, fighting its way through to a revolutionary view of issues which seem to force one to deal with them in a way which leaves them as perplexing as ever. Consequently, if one reads the *Investigations* in more than the most superficial way, one is driven, as Wittgenstein hoped, to think for oneself. By contrast, Waismann's is a textbook, in which the occasional insistence on the depth of his disquietudes, and remarks to the effect that 'to sense riddles and problems where others see before them the flat road of convention, is what constitutes the philosophic spirit' (p. 405) seems dangerously incongruous, because for the rest of the book everything seems to run very smoothly, and the tickets providing the answers emerge in a fairly effortless way.

This is related to the *length* of the book: it is about twice as long as the *Investigations*, but discusses, and prompts, far fewer questions. Treating the ones it *does* deal with in so protracted and relaxed a fashion, it gives the impression that one need not think more about them, that everything has been judiciously considered. This makes the reader more than usually passive, and the result is that, for most of the time, he is likely to be extremely bored. But what is more disturbing is that Waismann does not only dilute, in the sense of spreading much thinner, Wittgenstein's treatment of various fundamental topics, such as that of names, (where he even takes Wittgenstein's example of 'Moses'), but also in that he simplifies them and contrives to overlook precisely the most puzzling aspects of some of his problems. On pages 57-67, for example, he deals with the question as to why it is impossible for a surface to be both red and green all over. Essentially he gives the most straightforward conventionalist answer: the reason is that we have so decided to use words that if a surface is properly called red, it can not also properly be called green; if we wanted to, we could change the meanings of these words: 'Red (*sic*) might, for example, mean what is normally meant by "red or green"' (p. 62). And he continues in the same vein, without at any point considering the basic problem here, which is that once 'red' and 'green' are assigned the meanings they do have, it *follows* that we cannot apply both terms to the same patch. Why is this? What is the nature of the necessities which are generated by our definitions, without themselves being definitional?

Again, in dealing with the question of how we can know what another person is feeling, Waismann concludes; 'What makes it possible to communicate our subjective experiences is the fact that these experiences generally go together with certain definite bodily phenomena' (p. 259); one hopes so—but how we can *know* this to be the case in precisely the point in dispute.

All this is not to say that Waismann's book could not be of use: there are some chapters where his patience and lucidity would serve a beginning student well: twenty years ago we could all have learnt from it, but to anyone who has read the major works of the intervening period, it is bound to give the sensation of *déjà lu.*

I noticed only one serious misprint: on page 263, three lines from the bottom, 'subjunctive' should be 'subjective'.

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Rom Harré: *The Anticipation of Nature* (London, Hutchinson, 1965, 21*s*.) Pp. 108.

Mr Harré examines the various ways in which scientists and philosophers have tried to discover 'facts about the world' without having recourse to observation and experiment. He gives brief accounts of the doctrine of innate ideas, of Kant's principles of the pure understanding, of Cartesian mechanics, of Kepler's system of astronomy, of Eddington's physics, and of various proposed measures of the simplicity of hypotheses. He concludes that *a priori* reasoning can never guarantee the truth of statements about the world: that the anticipation of Nature is a fraud.

The quality of argument in the book is rather uneven. In places, for instance in the account of Locke's refutation of the doctrine of innate ideas, and in the précis of Kant's proof that we have synthetic *a priori* knowledge, the argument is impressively clear and concise, but elsewhere the dismissal of unwelcome views is both hurried and unfair. Thus (p. 104) he attacks Popper's association of the simplicity of hypotheses with generality and potential falsifiability, on the grounds that Popper has failed to discriminate between deductive generality, and taxonomic generality ('the more general covering more instances by saying less about each'). In fact (*The Logic of Scientific Discovery:* 36), Popper associates simplicity with deductive generality, and hence with degree of potential falsifiability, since the higher in a deductive system a hypothesis occurs the richer its logical consequences. He recognises two components of deductive generality in an hypothesis having the form of a universal implication: generality of the antecedent (level of universality), and precision of the consequent (degree of precision: the converse of taxonomic generality). Only by associating simplicity with taxonomic generality, the converse of Popper's criterion, can Mr Harré conclude that 'In those cases where generality is a mark of simplicity it is associated with the less potentially falsifiable statement', and hence reject Popper's suggestion on the grounds that it leads to contradiction. The subsequent dismissal of simplicity as 'at best of some pragmatic value, in the early stages of an investigation' is too sweeping. Even those who take a realist view of the status of scientific theories, as Mr Harré does, are faced with a problem in accounting for the selection of one hypothesis from the many hypotheses unrefuted, or equally well confirmed, by a given set of data. Similarly (p. 28), in discussing Strawson's *Individuals* he imputes to him the thesis that: 'there is a necessary connection between a particulate language and an individualist world', and is able to counter this thesis effectively. Surely this is a misunderstanding; at most Strawson claims to demonstrate a necessary connection between the structure of language and the structure of our conceptual scheme: between our use of a particulate language and our belief in the existence of individuals.

The balance of the book is upset by the disproportionately long and tortuous second chapter. We are led to expect that this chapter will examine the case for recognising a class of synthetic *a priori* propositions, but this cannot be so, for the opening paragraph characterises the analytic/synthetic distinction in such a way as to preclude the synthetic *a priori*. Various criteria of analyticity are then discussed in what the author describes as 'manoeuvres and mock-battles'. There is a clear exposition of the attacks of Quine and Waismann on the analytic/synthetic distinction. Quine's contention that both synonymy and necessity depend upon a prior notion of analyticity, and hence cannot be used to generate a class of analytic propositions wider than the tautologies, is left unchallenged. Yet Mr Harré goes on the recognise two kinds of necessity, as well as two kinds of analytic proposition, and two kinds of synthetic proposition—a veritable taxonomy of propositions. This complex prologue leads up to an examination of the case for recognising 'an independent class of necessary truths, which ... must characterise all possible worlds, but which it is hoped are not empty like tautologies'. Several apparent instances of such truths are considered and rejected. Since it is conceded (p. 52) that 'there are insuperable difficulties in identifying the class of necessary truth with any class of statement wider than the analytic', it would appear that any such truths must be analytic, but subsequently (p. 57) it seems that they are to be considered synthetic *a priori*. Both the purpose and the outcome of this chapter are obscure.

As an historical account of *a priori* reasoning in science this book could have been excellent: it is, perhaps, a pity that Mr Harré attempted so much more in so short a space.

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J. M. Burgers: *Experience and Conceptual Activity* (Cambridge, Mass., M.I.T. Press, 1965, $7.50, 57*s*.) Pp. x+277.

Professor J. M. Burgers, originally at the Technical University of Delft, is now at the Institute of Fluid Dynamics and Applied Mathematics in the University of Maryland. His special fields are fluid dynamics, magneto-dynamic flow, kinetic theory, and crystal dislocations, but he has also taken a keen interest in quantum mechanics, molecular biology and the philosophy of physics, biology and psychology. Moreoever for thirty years he has been a student of Whitehead's writings. He is thus exceptionally well fitted to provide, as he does in this book, a survey and extension of the more scientific aspect of Whitehead's philosophy. Compared with 'Process and Reality' Burgers's book has two advantages for most readers: it is written in a simple direct style, and it takes into account many recent scientific discoveries. A detailed analysis of the development of Whitehead's philosophical outlook is available elsewhere[1]; those who wish without undue effort to understand the essentials of Whitehead's 'philosophy of organism' (meaning, it seems, little more than emphasis on correlation and process or transformation) will find Burgers a valuable guide.

Whitehead believed that 'the most complete fact is dipolar, physical and mental'. Burgers accepts that position and uses it as his starting point for an analysis of the background of the sciences. For him 'conceptual activity' (a process involving anticipation, aim, choice, co-ordination, and value) is a basic feature of the entire universe. The current conception of physical causality, whether determined or statistical, does not exhaust the continuity of process. To account for the presence of life and of mind in this universe he considers that one must assume the presence everywhere of conceptual activity.

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[1] E.g. N. Lawrence: (Univ. of Cal. Press. 1956) *Whitehead's Philosophical Development. A critical history of the background of 'Process and Reality'.*

This view is unpopular today. But no one knows from what quarter and in what manner future generations may attain a deeper understanding of the awkward antithesis of conscious mind in a physical universe. Meantime those interested in this and kindred problems will welcome Burgers's analysis for its lucidity, even if, like the present reviewer, they cannot see that it is helpful to ascribe an element of mentality to all natural processes. For example, it might be enough if it were possible to recognise in the basis of a unified theory of particle-fields—to be expected sooner or later—not developed mentality, but the logical germ of life and of mind, i.e. the potentiality for developing organisms and mental processes during a long sequence of favourable circumstances.

But that is conjecture, and it is interesting to consider Burgers' views (all of them based on Whitehead's) on such matters a creativity, co-ordination in developing complexes of processes, societies of processes, matter, life, and organic evolution. Especially valuable is the section of fifty pages devoted to notes and references on many topics of current interest: entropy and order in the inorganic and organic, probability and statistics, chemical evolution, co-operative phenomena, information theory, co-ordinative processes, directiveness, self-organising systems, etc.

In my view Whitehead would have welcomed this book and perhaps have recognised it as a simpler and, in one sense, more advanced expression of the scientific essence of his own philosophy of organism. It has been suggested that the obscurity of 'Process and Reality' arose from a premature desire for comprehensiveness. However that may be, Burgers never fails to make his meaning clear.

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Peter Caws: *The Philosophy of Science: A Systematic Account.* (New Jersey, D. van Nostrand, 1965, 52*s.* 6*d.*) Pp. ix+354.

Unlike many books often so described, this is an introductory work. Dr Caws neither assumes his reader has any knowledge of philosophy or logic nor attempts to solve any of the problems he deals with in a way which a more sophisticated reader would find acceptable. Instead he has written forty-four generally very short chapters introducing and discussing the main problems in the field so that it should be clear to a student why they arise and how they are related. The book is divided into four parts dealing with the relation of language to experience and the world, the logical structure of theories, the validation of theories and 'the adequacy of the account of the world which the (scientific theories) present to our belief' (p. vii). The first part contains a somewhat idiosyncratic account of thought and language, but, it does not greatly affect the usefulness of what follows in the next two parts. Here, the reader is taken through a clear account of the hypothetico-deductive model of theories, the problems of evidential support, the nature and interpretation of probability statements, and the problem of induction.

In the Preface, the author writes: 'No claim is made to novelty, or even to thoroughness, in the treatment of the various topics covered. But what there is is systematically arranged, and this will perhaps be found useful by readers who are looking for some orientation in this untidy discipline' (p.vi). Those who believe that learning to be thorough is an essential part of learning philosophy, probably will not like this book, not because Dr Caws does not go into all the ramifications of each of the topics he deals with, but because in many chapters problems are glossed over and dubious arguments given in the attempt to carry the reader through to an overall grasp of the subject. But even those who feel that there is a need for a book of this kind will possibly find that two major faults detract from the value of this one. First, the author rarely discusses alternative positions to the one he is advocating, and where he does so, there is little detail given. This could easily lead a student to believe either that there is no alternative or that there is, but it is obviously unacceptable. Perhaps the most surprising omission is any mention of Campbell in the chapter on Models, where an account similar to Braithwaite's is given, beginning: 'The notion of a "model" . . . is one of the most important in the philosophy of science' (p. 139). Secondly, although references are given at the end of each chapter, no bibliography is given for each topic or suggestions for further reading. Generally his references are hardly sufficient to aid the reader. For example, no one could hope to do more in ten pages than give a very rough introduction to deductive logic and yet the references at the end of this chapter are to Aristotle's *Organon*, Mates's *Stoic Logic*, Frege, Russell and Whitehead, and Boole.

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A. Campbell Garnett: *The Perceptual Process*. (Madison, Wisconsin, University of Wisconsin Press, 1965, $3.75.) Pp. 104.

Professor Garnett's book begins with an introductory chapter on 'The Task of Philosophy'. This contains three theses: (1) that the analysis of mental life is the most important task of the philosopher; (2) that the analysis of ordinary language is an extremely valuable tool for this task, though it generally needs supplementation in various other ways; (3) that the failures of 'scientific' introspection (author's quotes) and the results of psychoanalysis do not imply that progress cannot be made with (1) 'by the philosopher's armchair method of reflection on personal experience'. It is doubtful whether the arguments offered on behalf of (1) and (2) at least are adequate. For example, (1) is justified by the assertion that 'all the various departments of philosophy are analyses of different parts or phases of mental life'. Thus 'ontology is an examination of the concept of being and an attempt to determine its most general categories as found in experience and assumed in thought' (p. 16). But it is far from clear that, for example, because the world we experience is spatiotemporal, therefore the analysis of the concepts of space and time comes under the heading 'analysis of mental life'. Such an argument does not show that philosophy necessarily has any uniquely close concern with the analysis of mental life, if the latter is understood in an ordinarily restricted sense.

The main part of the book consists of three chapters containing 'an attempt to analyse the perceptual consciousness and formulate a theory of the relation of our experience of observation to what we find in the course of observation and what we believe to exist independently of observation' (Preface). 'The theory presented is in the general tradition of the British realists, particularly Broad, Stout, Whitehead, though it profits considerably by the work of the linguistic analysts, especially Ayer, Ryle, and Austin, while critically differing from them.' (Ibid.)

Chapter II is entitled 'Consciousness and Observation'. The following views are defended. (1) Observing, noticing, and in general what Ryle calls the 'minding' concepts ('heeding', 'attending', etc.) are terms that refer to a mental act or performance; that is, they are 'episodic' rather than 'dispositional' terms. (2) Observing involves (i) having various items of experience, (ii) noticing some particular item, (iii) trying to find out something more about it. (3) We notice at least (i) bodily sensations and feelings, (ii) sense *qualia*, and relations between them, and, (iii) 'physical things', 'material objects'. A further thesis is asserted, viz. (4) that the 'minding' concepts 'refer to mental activities or events which, in their interconnection, constitute a mind'. A mind is a more or less well organised system of minding' (p. 36). This is not argued for here, and though it appears at various places (e.g. pp. 40 and 99 f.) it is never really explicated or defended in much more detail.

Chapter III ('Sensations and Things') begins with a specification of the concept of a physical object. This is followed by a discussion, along fairly familiar lines, of naive realism and the theories of representative perception, the argument from illusion and phenomenalism. The last portion of the chapter is an extended discussion of Austin's attack on the sense-data theory. Professor Garnett thinks that this is unsuccessful in its main purpose, but does bring out a central inadequacy of sense-datum language, viz. its inadequacy for expressing the fact that the use of verbs of perception involves claims not only about immediately present sense *qualia* but also that the latter stand in some relation to 'things'.

Chapter IV ('Obstacles and Symbols') takes up the question of the character of 'things'. It is said (1) that a thing is essentially 'a potential *obstacle*' (p. 74), a 'potential centre of resistance' (p. 75). (To the list of precursors of a view of this sort given on page 74 might be added Wilhelm Dilthey—see his *Gesammelte Schriften*, vol. V, esp. pp. 98 ff.) It is then argued that things conceived in this way satisfy the concept of a material object, as specified at the beginning of chapter III. In the course of the discussion of the fifth condition (viz. interaction). Hume's account of the source of our idea of necessary causal connection and physical interaction is criticised at some length (pp. 84-89, 91 f.)

I am still unclear about the way in which the notion of a 'thing' as an 'obstacle', potential or actual, is supposed to solve any of the problems about the external world which have been exhibited, though it might possibly serve as a causal explanation of our belief in the existence of things. A very natural query is this. Qualia are said to be immediately present to consciousness in experience. (The term 'quale' is, by the way, used without special introduction. It may be noted, however, that it appears to be decided in the end that *qualia* are mental in character —see pp. 97 f.) We certainly have experiences of being obstructed. But why are these not to be interpreted simply as experiences of further *qualia* (e.g. pressure, hardness) and not as vouchsafing access to something other than qualia, some 'non-sensory elements' (pp. 84 and cf. pp. 76, 86) to which qualia relate? If they are interpreted in the first way then the author is left with a version of the causal theory of perception in which he attempts, unsuccessfully, to bridge the famous gap between 'ideas', 'sense-data', etc., and their material causes, not by some form of inference, but by a *practical* relation (cf. p. 93). The crucial point here is clearly to show that our experience of obstacles cannot be accounted for in terms of sensations of pressure, etc. The author does provide some argument on this point (pp. 77-80, 88 f., 92), but I have not succeeded in following it.

(2) Secondly, it is said that the sensory *qualia* function in *symbolic* relations with the conditions in the physical world which consists of our own bodies and other physical things. This they do by becoming associated by habit with potential centres of resistance of different sorts which are actualised when we make the appropriate efforts in the places and times suggested by the *qualia*.

(3) Finally, 'the mystery of the relation of body and mind reduces to that of the one ultimate two-way relation which we must accept as a plain fact of experience —the operation in which efforts to do something find resistances and find some of them partially controllable and others not, and the emergence in return of feelings and other sensations which symbolise for us the occurrence of, and changes in the organisation of, centres of resistance, particularly in that complex and highly controllable one we call our body' (p. 98).

In sum, if we except chapter II, which has been largely published already in *Mind* (July 1960), chapter IV is probably the only part of the book which may contain something novel for the professional philosopher. But, as has been just briefly indicated, there is a good deal in it which requires clarification and much that is questionable.

I have noticed the following typographical errors: 'principle' for 'principal', page 12, line 3; '(3)' for '(4)', page 48, last line.

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H. A. Buchdahl: *The Concepts of Classical Thermodynamics* (Cambridge, at the University Press, 1966, 45*s*.) Pp. xi+223.

Professor Buchdahl's aim is to present the basic ideas of classical thermodynamics on a purely phenomenological level, against a general background of physical theory. Two-thirds of the book is occupied with a discussion of the fundamental laws. The author's preference is for a treatment that is (i) analytic, but not axiomatic, because the axiomatic treatment of a physical theory may too easily give the impression that it is simply an exercise in formal logic, rather than an interpretation of observations; (ii) phenomenological, not concerned with molecular interpretations, because such a concept as entropy can be understood without the statistical-molecular approach. This is none the less a theoretical physicist's book, in which the mathematical and logical treatment is sophisticated and thorough.

The part of the book that will be of greatest interest to readers of this *Journal* is the first chapter, which lays down the definitions and conceptual tools used as foundation for the superstructure. Here will be found discussions of operational definition, of the relation between physical and mathematical notations, and of the relation between thermodynamic (phenomenological) theory and its interpretation by the statistical treatment of assemblies of molecules. The treatment is concise, clear-headed and rigorous. There follow chapters on the zeroth law, first law, second law (treated by the method of Carathéodory), third law (the impossibility of attaining the absolute zero of temperature), and thermodynamic equilibrium. Though abstract, the treatment never loses contact with the results of experiment. This is not a book for beginners, nor for those whose interest in thermodynamics is purely experimental; for those with the right mathematical and physical equipment, it will be of considerable interest.

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Abstracts

Dialectica, **20** (1966), No. 1.

G. Bouligand, 'Le philosophe et le savant unis dans l'oeuvre epistemologique'.

Dans l'oeuvre de Ferdinand Gonseth, on voit culminer le désir de rapprocher, dans leurs objectifs et dans leurs méthodes, le philosophe et le savant.

Cette idée même a guidé, compte tenu des exigences historiques, les développements qui précèdent.

Après un parcours rapide, évoquant les vingt premières années de *Dialectica*, le rôle scientifique joué dans cet imposant répertoire par son fondateur, rôle fortement accru par ses livres, on trouvera donc une enquête sur des thèmes favoris dont l'intérêt s'est confirmé, et dont les objets ont une haute valeur, pour qui voudrait analyser les courants de pensée capables d'enrichir la philosophie scientifique et d'affecter utilement le proche avenir de *Dialectica*.

P. Bernays, 'Gedanken zu Buch "Bildung und Mathematik (Mathematik als exemplarisches Gymnasialfach)" '

Alexander Wittenberg's book 'Bildung und Mathematik (Mathematik als exemplarisches Gymnasialfach)' contributes to the actual discussion of the question how teaching of mathematics at the gymnasium should best be performed. Wittenberg had already devoted various publications to this subject. It was his intentio nto continue the present work by a second part; however he has been, alas, prevented from this by a premature death.

The leading idea in Wittenberg's dealing with the said question is, that one should not be concerned at first for the preparation of later studies but rather for the formation of the personality. Concerning in particular mathematics, he wants to show that this field is especially suitable for such a teaching. Wittenberg's argument in his book consists mainly in the following: (1) he points to those characteristics of mathematics which are particularly favourable for procuring a wider mental horizon; (2) he offers some general suggestions for the method of teaching, in particular the 'Themenkreismethode'; (3) he presents, as an instance, the description of a course of elementary geometry. The course is arranged in a way to favour the development of creative and independent thinking. In stressing this concern Wittenberg is in agreement with the tendencies uttered by several authors of our time, as M. Wagenschein, G. Polya, M. Wertheimer, A. N. Whitehead. Wittenberg's way of writing is very impressive and his book abounds in instructive observations and suggestive remarks.

The present article is a report of Wittenberg's book containing in particular a brief description of the said course in elementary geometry, with some commenting remarks.

P. E. Pilet, 'L'analogie en biologie'

Usual definitions and relative importance of the analogy—in biological sciences—are first discussed. Analogy-conclusion, analogy-hypothesis and analogy-invention are then successively presented in relation to the descriptive (chiefly taxonomy and cytology) and experimental (physiology and biochemistry) biology.

H. König, 'Uber ein einfaches Drei-Komponenten-Modell fur das menschliche Denken'

Anfangspunkist die Arbeitshypothese, wonach man sowohl von der Subjekt-Seite wie von der Objekt-Seite aus das geistige Verhalten des Menschen summarisch in drei Komponenten zerlegen könne, die wir subjektseitig Potenzen, objektseitig Valenzen nennen. Die drei Potenzen und die drei Valenzen begegnen sich in neun 'Aktivitäten', oder Teilfeldern des geistigen Geschehens. Das Gesamtfeld betrachten wir als eine in sich geschlossene Mannigfaltigkeit, die dem 'Feld' der Gestaltpsychologie entspricht. Wenn man sich über die Reihenfolge, in der die neun Teilfelder von elektrophysiologischen Prozessen durchlaufen werden, gewisse einfache Vorstellungen macht, ist man in der Lage, (1) in diesem Modell zu erklären, wo die Grenze zwischen unbewusst und bewusst liegt; (2) zu deuten, warum es verschiedene Charaktertypen gibt; (3) eine Synthese zwischen Wissen und Glauben anzubahnen; (4) ein cybernetisches Schema für Klassifikationszwecke aufzustellen. Das skizzierte Modell erscheint also recht leistungsfähig zu sein und gestattet mancherlei Anwendungen.

C. T. K. Chari, 'Information theory, quantum mechanics and "linguistic duality" '

The paper explores first the postulational basis and significance of 'measures of information' in current information theory and their possible relations to physical entropy and Brillouin's 'negentropy' regarded as the negative of entropy. For some purposes, the same pattern or formal structure may be abstracted from both 'entropy' and 'information'. The paper analyses, in the second place, the mathematical analogies which have been traced between information theory and quantum mechanics and argues that the analogies have but a limited value when we come to grips with the deeper, and yet unsolved, problems of quantum theory. Lastly it is urged that the indiscriminate extensions of statistical and information-theoretic methods, and of quantum-mechanical analogies, e.g. 'linguistic duality', to psycholinguistics, literary and aesthetic *Gestalten*, can result only in crude exaggerations. Dialectical methodology requires the continual reformulation of all scientific descriptions which have to reckon perpetually with richness of human materials.

Philosophy of Science, 32 (1965), No. 3.

Ernest W. Adams, 'Elements of a Theory of Inexact Measurement'

Modifications of current theories of ordinal, interval and extensive measurement are presented, which aim to accommodate the empirical fact that perfectly exact measurement is not possible (which is inconsistent with current theories). The modification consists in dropping the assumption that *equality* (in measure) is observable, but continuing to assume that *inequality* (greater or lesser) can be observed. The modifications are formulated mathematically, and the central problems of formal measurement theory—the existence and uniqueness of numerical measures consistent with data—are re-examined. Some results also are given on a problem which does not arise in current theories: namely that of determining *limits of accuracy* attainable on the basis of observations.

Richard E. Robinson, 'Measurement and Statistics: Towards a Clarification of the Theory of "Permissible Statistics" '

Much of the criticism of Stevens's criterion for permissible statistics as applied to measurement data results from a lack of clarity in Stevens's position. In this paper set-theoretical notions have been used to clarify that position. We define a *sig-function* as a function defined on numerical assignments. If $\mathscr{U}$ and $\mathscr{R}$ are empirical and numerical relational systems, respectively, then a sig-function F is *constant* on $\mathscr{U}$ with respect to $\mathscr{R}$ if, and only if, the value of F is the same for all numerical assignments for $\mathscr{U}$ with respect to $\mathscr{R}$. Using these notions we prove rigorously certain generalisations of Stevens's results.

Edward Manier, 'The Theory of Evolution as Personal Knowledge'

Dr Marjorie Grene has argued that criteria taken from a personalist philosophy of science have regulative force in the dispute between orthogenetic and synthetic or neo-Darwinian theories of evolution, and that these criteria commend the acceptance of the orthogenetic position. Grene's position includes two basically correct theses concerning the limitations of operationism and reductionism. However she fails to show that personalist tenets are necessary for the validation of these two theses. Moreover, the proposed modifications of evolutionary theory depend upon additional premisses: that biology must study individuals rather than populations, and that the synthetic theory must prove that natural selection and mutation are the only possible factors for control of the direction of evolutionary change. The evidence for these premisses is called into question.

Brian Skyrms, 'On Failing to Vindicate Induction'

The structure of Reichenbach's pragmatic vindication of induction is analysed in detail. The argument is seen to proceed in two stages, the first being a pragmatic justification of the frequency interpretation of probability which is taken as a license for considering the aim of induction to be the discovery of limiting relative frequencies, and the second being the pragmatic justification of induction itself. Both justifications are found to contain flaws, and the arguments used to support Reichenbach's definition of the aim of induction presuppose the availability of a type of predicate which generates paradoxes closely related to Goodman's 'grue-bleen' paradox.

Next, Salmon's 'Criterion of Linguistic Invariance' is evaluated as a canon of inductive logic, which singles out the 'straight rule' from an infinite class of convergent inductive rules. Upon close examination, its credentials are seen to be unimpressive. In connection with Salmon's work on linguistic invariance, we take a closer look at the impact of the Goodman paradox on probability estimator rules like Reichenbach's 'straight rule'. We find that its undesirable consequences for such systems of inductive logic cannot be escaped by Salmon's solution, or indeed by any solution whose motivating idea is to rule 'queer' predicates out of court.

Finally, the discussion of the Goodman paradox leads us to a modest methodological proposal for systems of inductive logic which incorporate probability estimator rules of the type at issue.

Kenneth C. Clatterbaugh, 'General Ontology and the Principle of Acquaintance'

What one is acquainted with has always been important for the rejection or acceptance of any ontological description. Yet the relevance of acquaintance to ontology has not always been clearly stated. Some philosophers have held that they were acquainted with the simple entities of ontological analysis. They also held that if they were not acquainted with such entities, their analysis would be inadequately supported. In this paper I argue that acquaintance with ontological simples cannot be a reason for accepting or rejecting any ontological analysis. At the same time, I examine what I believe to be the relevance of acquaintance to ontological description.

Philosophy of Science, 32 (1965), No. 4

R. Duncan Luce, 'A "Fundamental" Axiomatization of Multiplicative Power Relations Among Three Variables'

Suppose that entities composed of two independent components are qualitatively ordered by a relation that satisfies the axioms of conjoint measurement. Suppose, in addition, that each component has a concatenation operation that, together either with the ordering induced on the component by the conjoint ordering or with its converse, satisfies the axioms of extensive measurement. Without further assumptions,nothing can be said about the relation between the numerical scales constructed from the two measurement theories except that they are strictly monotonic. An axiom is stated that relates the two types of measurement theories, seems to cover all cases of interest in physics, and is sufficient to establish that (the multiplicative form of) the conjoint measurement scales are power functions of the extensive measurement scales.

R. F. Tredwell, 'The Problem of Counterfactuals'

The 'problem of counterfactuals', as proposed by Goodman and Chisholm, cannot be solved. However, a similar programme, pioneered by Hiz and Mrs Milmed, but largely neglected, can be completed and promises a satisfactory analysis of subjunctive conditionals.

John A. Winnie, 'Theoretical Terms and Partial Definitions'

The problem of the interpretation of theoretical terms is outlined, and some difficulties connected with the distinction between partial definitions and empirical postulates are discussed. A reconstruction is sketched which is intended to explicate the 'definitional' character of partial definitions. Finally, some implications for the methodology of theory construction are indicated.

Michael David Resnik, 'Frege's Theory of Incomplete Entities'

This paper examines four arguments in support of Frege's theory of incomplete entities, the heart of his semantics and ontology. Two of these arguments are based upon Frege's contributions to the foundations of mathematics. These are shown to be question-begging. Two are based upon Frege's solution to the problem of the relation of language to thought and reality. They are metaphysical in nature and they force Frege to maintain a theory of types. The latter puts his theory of incomplete entities in the paradoxical position of maintaining that it is no theory at all. Moreover, his metaphysics rules out well-known suggestions for avoiding this difficulty.

Gerald J. Massey, 'Four Simple Systems of Modal Propositional Logic'

Four progressively ambitious systems of modal propositional logic are set forth, together with decision procedures. The simultaneous employment of parenthesis notation and parenthesis-free notation, the dual use of symbols as primitive and defined, and the introduction of a new modal operator (the truth operator) are the principal devices used to effect the development of these logics. The first two logics turn out to be 'the same' as two of von Wright's systems.

Constantine Politis, 'Limitations of Formalization'

After several decades during which formalisation has flourished it now becomes possible to detect its shortcomings. A definition of formalisation is given at the outset. It is next shown that the main justification of formalisation as making explicit the form of a proof has serious difficulties. An important shortcoming is found in the fact that many validation procedures in logic and mathematics are not adequately represented deductively. Several such procedures relating to the validation of logical and mathematical sentences are examined. It is concluded that formalisation is materially inadequate.

Robert Barrett, 'Quine, Synonymy and Logical Truth'

W. V. O. Quine's well-known attack upon the analytic-synthetic distinction is held to affect only one of the two species of analytic statements he distinguishes. In particular it is not directed at and does not affect the so-called logical truths. In this paper the scope of Quine's attack is extended so as to embrace the logical truths as well. It is shown that the unclarifiability of the notion of 'synonymy' deprives us not only of 'analytic statements that are obtainable from logical truths by the replacement of synonyms with synonyms' but of 'logical truths' themselves.

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A Defence of the Principle of Event Causality*

by MILTON FISK

Advocates of the principle of causality are viewed, both by themselves and by others, as subject to serious attack from quantum theory. Thinking themselves beleaguered, they might take comfort from the efforts of Bohm[1] and others to reinstate causality in microphysics. But, on the one hand, these efforts have not, in the fifteen years since they began, produced a viable alternative to the standard quantum theory. On the other hand, it has been, as I shall try to prove in detail, a mistake even to think the traditional principle of causality is subject to attack from quantum theory. What is subject to attack is a much stronger principle, which, as it happens, interpreters of science have substituted for the traditional principle. By emphasising the difference between the new principle and the old, I shall hope to make clear that facts of the sort currently adduced as counting against the new principle in no way count against the old. The conclusion will then be that there is no more reason now to believe that there are spontaneous happenings than there was before quantum theory.

1. EVENTS AND VALUES

By the traditional principle of causality I mean the principle that every event has a cause. I shall refer to this principle as the principle of event causality. The new principle which has been substituted for it is expressible with the idea of a value of a functor of a thing. 'Thing' here is broadly understood so as to apply to physical objects, events, and conditions of physical objects and events. 'Functor' is to be used as follows. In the sentence 'The colour of the chair is black', the expression 'the colour of' signifies a functor. With the particular chair in question as argument, the property black is the value of this functor. Thus in 'The temperature of the rock is ten degrees centigrade', 'the temperature of' signifies a functor, which has the property ten-degrees-centigrade as its value for the rock in question as argument. The idea of a functor is then a straightforward generalization of that of a numerical function. But the full generality of the idea of a functor is not wanted here. For, the new principle is not that any value which any functor of any thing assumes has a cause, but that there is a cause of any value assumed by any *physical* functor with *properties* as values for any thing as argument. The point of each of the two restrictions can be brought out as follows. First, an argument of a *nonphysical* functor, like that expressed by 'the signification of', is not dependent for its being fully caused on whether the corresponding value is caused. Second, an argument of a functor with *nonproperties* as values, like that expressed by 'the neighbour of', is not in general dependent for its being fully caused on whether the corresponding value is caused.

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* Received 14.iv.66

[1] Cf. D. Bohm, 'Hidden Variables in the Quantum Theory', in *Quantum Theory*, vol. III, ed. D. R. Bates, Academic Press, New York, 1962, pp. 345–87; D. Bohm, *Causality and Chance in Modern Physics*, Harper, New York, 1961, chapter 4; and *Observation and Interpretation in the Philosophy of Physics*, ed. S. Körner, Dover, New York, 1962, Second Session.

Strictly speaking, values of property-functors are not caused. Neither one meter from the origin, ten degrees centigrade, nor five seconds after the start is caused. Rather, it is the event of the ball's hitting the wall at one meter from the origin, the rock's condition of having a temperature of ten degrees centigrade, and the event of decay five seconds after the start which can be caused. To give prominence to the values in these possible effects, let us redescribe them with the formula 'value=functor (argument)' in mind:

(1) one-meter-from-the-origin's being the place the ball hits the wall,
(2) ten-degrees-centigrade's being the temperature the rock has,
(3) five-seconds-after-the-start's being the time the decay occurs.

The expression '*x* was the cause of' can be applied to (1)-(3) to obtain causal propositional forms which are perfectly in order. But what kind of entity is *x* the cause of when it is the cause of some property's being the value of a functor with some definite thing as argument? It is not an event, nor is it a fact in the sense in which fact is wedded to that-clauses. But this is not the only sense which 'fact' has; the entities named by (1)-(3) are facts, though facts-of rather than facts-that. Since these particular facts are identifications of a value as the value of a functor of a thing, I shall call them *value-facts*. The new principle is then more properly the principle that every value-fact constituted by a physical functor with properties as values has a cause. This will be called the principle of value causality.

Some values are the values of functors of events, others are not. One meter from the origin is the value of the place-functor of the event of the ball's hitting the wall. But the rock is not an event, and hence, in (2), ten degrees is not the value of a functor of an event. Even in those cases in which the value is the value of a functor of an event, neither the value itself nor the value-fact is an event. By *events* I shall mean those and only those things of which it can be said that they occur, happen, or take place. It is well to point out the following features of events as so characterised. As things which occur, happen, or take place, events have dates. But the dates on which or at which events take place can be more or less precise; a killing might not be specifiable more precisely than as having taken place on a certain day, say on 1st June 1965, whereas the shooting involved in the killing can be specified as having taken place at a certain minute, say at 10.03 p.m. In addition it is easy to confuse an event with the changing or acting which is constitutive of it, since often the same expression is used for both. The running can be either that which takes place this afternoon or that which wears Peter out, which gets slower on the third lap, and which begins at 4.00 p.m. and lasts four minutes. The running as an event does not itself change, begin, or endure; only the running as that which Peter and the others are doing can change, begin, or endure. Further, it is clear that events as understood here are not short temporal slices of enduring objects, as they are sometimes understood to be. For, Peter-at-*t* is on a par with Peter in that neither takes place. Finally, since events are things which happen or occur, a thing's happening *to be the case* or occurring *to me* does not qualify it for eventhood. That the cream is sour happens to be the case, but does not happen, and hence is not an event. That π might not be an algebraic number occurs to me, but does not occur, and hence is not an event. Correspondingly, satisfying the context 'it happens that . . .' or 'it occurs to me that . . .' serves as a criterion for being a fact-that designator and a proposition designator, respectively, but not for being an event designator.

If one replaces 'cause' by a definition of it, one gets formulations related to those above of the principle of event causality and of value causality, but somewhat more complex in nature. Using the Humean definition of cause,[1] the principle of event causality becomes: For any event e_1 there is an event e_2 such that e_2 is followed by e_1 and events like e_2 are regularly followed by ones like e_1. So formulated the principle of event causality holds only if the principle that the same cause is always associated with the same effect holds. The latter principle I shall call the strict homeoetic principle. It is not to be confused with the principle of event causality since it may be true even if some events lack causes. If, however, we assume, as I shall here, that no replacement is made for the word 'cause' in the principle of event causality, we are not explicitly committed to the strict homeoetic principle. But first, it will be helpful to consider the consequences of certain other definitions. The Kantian definition of a cause as that on which something follows according to a rule gives the principle of event causality in the form: ‘Everything that happens, that is, begins to be, presupposes something upon which it follows according to a rule.’[1] Since the rule may relate not the cause alone but the cause in a specified set of circumstances to a single effect, the strict homeoetic principle is not implied. Yet, in view of the inherent generality of a rule, at least the principle that the same cause in the same circumstances leads to the same effect is implied. This is the weak homeoetic principle. Now the principle of value causality is often formulated with the aid of a definition of cause in terms of prediction. According to the definition, ‘has a cause’ means the same as ‘is such that there are laws and previous conditions in terms of which the value-fact in question could be predicted’. With this definition, the principle of value causation becomes: Every value-fact (constituted by a physical property-functor—a qualification which henceforth shall remain implicit) is such that there are laws and previous conditions in terms of which it could be predicted. Because of the importance of this form of the principle of value causation, I shall give it a special label, the principle of prediction. Each of the following has a strong family resemblance to the principle of prediction: ‘. . . natural phenomena obey exact laws’,[2] ‘The initial state determines the future according to the laws of nature’,[3] and ‘. . . we can predict the future with a determinate probability and . . . we can push this probability as close to certainty as we want’.[4] However, Margenau’s principle, ‘Let A and B be complete states of a specified system at times t_1 and t_2, t_1 being earlier than t_2. If A is realised, B will certainly follow’, is essentially different from the principle of prediction since his states, unlike our value-facts or sets of our value-facts, are defined as just those sets of features on which precise predictions by laws are based and which are predicted precisely by laws.[1] With this definition, Margenau's principle is an analytic truth, whereas the truth, if it is a truth, of the principle of prediction does not follow from the bare definition of value-fact given above.

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[1] Hume, *Treatise*, I, III, XIV, *ad fin.*

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[1] Kant, *The Critique of Pure Reason*, A189. The following are some variants of the principle of event causality with *cause* unreplaced by a definition: ‘All that becomes must needs become by the agency of some cause’ (Plato, *Timaeus*, 28A); ‘All things in motion must be moved by something’ (Aristotle, *Physics*, 256a2); ‘That which does not exist begins to exist only through something already existing’ (Aquinas, *Summa Theologica*, I, 2, 3c); and ‘Whatever begins to exist must have a cause of existence’ (Hume, *Treatise*, I, III, III). Cicero comes closest to our principle of event causality by capturing the full scope of *event* in “. . . everything takes place (*fiunt*) with antecedent causes’ (*De Fato*, 31) and in ‘. . . nothing can happen (*evenire*) without an antecedent cause’ (ibid. 34).

[2] W. Heisenberg, *The Physical Principles of Quantum Theory*, trans. C. Eckart and F. C. Hoyt, University of Chicago Press, Chicago, 1930, p. 62.

[3] M. Born, *The Restless Universe*, trans. W. M. Deans, Dover, New York, 1951, p. 156.

[4] H. Reichenbach, *Philosophical Foundations of Quantum Mechanics*, University of California Press, Berkeley, 1965, pp. 2-3. Heisenberg, Born, and Reichenbach give the principle of prediction in the quoted forms, only to reject it in the light of quantum theory.

Historically, the substitution of the principle of value causality for the principle of event causality stemmed, in large part, from the great advances made in the discovery of more and more precise laws. With such laws it became possible to refer to events in more and more specific ways in giving causal accounts of them. One could not only say both that something's happening was caused and that the ball's hitting something was caused but also that the ball's hitting the wall was caused and even that its hitting the wall precisely at x was caused. So long as all of this is summarised merely by saying the event is caused, no indication is given of the great access to causal knowledge due to the discovery of new laws. To indicate this and at the same time to express the promise it seems to carry of complete causal knowledge, one will say that all of the value-facts associated with the event are caused. The statement that this event is caused is true merely if there is a cause which, in this case, makes the difference between something's happening and nothing's happening, and even though this cause does not make for a difference between a hitting and something which is not a hitting, a hitting of the wall and a hitting of the door, and a hitting at precisely x and a hitting at precisely y. Just as the statement that this event is caused comes to be replaced by the more daring statement that every value-fact associated with this event is caused, so too the principle of event causality comes to be replaced by the principle of value causality.

Galileo, as in many things, stood at the midpoint in this transition too. He recognised that one might fail to account causally for the fine details of events even while succeeding in accounting causally for these events as events specified in terms of certain broader features. Thus he took cognisance of event causality. But at the same time he affirmed the importance, and the difficulty, of finding causal accounts of the fine details:

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[1] H. Margenau, *The Nature of Physical Reality*, McGraw-Hill, New York, 1950, pp. 393-4. Since states are defined as the bases for predictions by laws and as themselves predictable by laws, the problem of causality is no longer whether states are caused, but whether every system has at some time—and hence at every time—a state. So, for Margenau, the analogue of the principle of prediction is not 'States are predictable', which is true by definition, but 'Every system has a state'. This is, however, weaker than 'Every value-fact is predictable', that is, the principle of prediction itself. For, eigenvalues of quantum mechanical observables are not in general predictable, but systems with these observables have states—expressible as ψ-functions—, which are predictable since they satisfy the Schrödinger time equation.

> This is all I can tell you about the matter, and perhaps it is as much as can be comprehended within our knowledge—which, as is well known, can be only of such conclusions as are fixed and constant. Such are the three general periods of the tides, since these depend upon invariable causes which are unified and eternal. But with these primary and universal causes are mixed others which, though secondary and particular, are capable of making great alterations; and these secondary causes are partly variable and not subject to observations (the changes due to the winds, for example), and partly, though determinate and fixed, are not observed because of their complication. Such are the lengths of the sea basins ... and the many and various depths of the waters. ... The precise durations of the ebbing and flowing are changed ... by the lengths and depths of the basins.[1]

On the view of universal determinism there will, of course, always be secondary causes to account for the fine details. Even so, the distinction stands between, on the one hand, primary causes and general features and, on the other hand, secondary causes and fine details. This distinction does not call universal determinism into question. It tells us only that the existence of spontaneous events cannot be inferred from the premiss that there are uncaused fine details of events. Galileo's distinction challenges such an inference even though it is a distinction which is relative to context; general features in one context may be fine details in another. In what follows, I shall attempt to set out the logic of the relation between events and their details, both fine and broad, in order to reach maximum clarity on the implications of causal failures of various kinds for the principle of event causality.

2. THE AMBIGUITY OF EFFECT EXPRESSIONS

Suppose there is a cause of the event of the ball's hitting the wall. If the question is 'Who or what made the ball hit the wall?' it might be that

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[1] Galileo, *Dialogue Concerning the Two Chief World Systems*, trans. S. Drake, University of California Press, Berkeley, 1962, p. 460. Cf. also Hume, *Treatise*, I, III, XI, where the probability of something which is a matter of chance, and hence without a cause, is said to rest upon there being a cause of something else, related to the first thing somewhat as the general is to the particular. (Here, of course, when Hume speaks of something without a cause he is speaking of what he calls the natural relation of causation.) ' 'Tis suppos'd, tho' the dye be necessarily determin'd to fall, and turn up one of its sides, yet there is nothing to fix the particular side, but that this is determin'd entirely by chance. .... When therefore the thought is determin'd by the causes to consider the dye as falling and turning up one of its sides, the chances present all these sides as equal, and make us consider every one of them, one after another, as alike probable and possible. .... 'Tis after this manner the original impulse, and consequently the vivacity of thought, arising from the causes, is divided and split in pieces, by the intermingled chances.' The event, the turning up of a side, is, as thus described, caused, though by supposition, one of its details, a value-fact, two's being the number of the side turned up, is uncaused. Hume would, in principle, have no difficulty saying that the motion of a photon after it reaches the surface of a crystal is caused even though its motion's being a reflection or a transmission is uncaused.

Jones is the cause; if it is rather 'What was done to make the ball hit the wall?' it might be that Jones's throwing the ball is the cause. But suppose further that, however the question is framed, there is no cause of the event's occurring at place *x*. So the value-fact of *x*'s being the place the ball hit the wall is not caused. The question which naturally arises is this: Does not the second supposition lead directly to there being an uncaused event, namely, the event of the ball's hitting the wall at *x*, even though by the first supposition the event of the ball's hitting the wall is caused? It then seems impossible to hold that the principle of value causality could fail without a corresponding failure of the principle of event causality.

To reply to this objection I appeal to an evident ambiguity of propositions of the form 'The ball's hitting the wall at *x* is uncaused'. A singular term of the form:

(4) the ball's hitting the wall at *x*

can mean the same as:

(5) the-hitting-of-the-wall-by-the-ball which occurred at *x*

or it can mean the same as:

(6) the-hitting-of-the-wall-by-the-ball's occurring at *x*.

On interpretation (5), the 'at *x*' in (4) serves only to identify which event it is that is being referred to. The event can be caused even though the feature of it by which it is identified is not. On interpretation (6), (4) is no longer an event-name at all, for it is not a name of the event of the hitting but a name of its occurring at *x*. If it were an event-name, it would be so by the fact that occurrences could occur. But if this were a fact then involved in the occurrence of a single event would be an infinity of occurrences. Actually, (6) is just a way of expressing the value-fact:

(7) *x*'s being the place the hitting of the wall by the ball occurs.

It follows that the ball's hitting the wall at *x* is uncaused only under interpretation (6). But this interpretation does not give us an uncaused event but an uncaused value-fact, as is clear from the equivalence of (6) and (7). Under interpretation (5), the ball's hitting the wall at *x* is caused, if it is true that the ball's hitting the wall is caused.

I shall try to strengthen the point that the failure of value-fact causality does not entail the failure of event causality by consideration of two more hypothetical examples. Suppose a rock always changes temperature when it is exposed to the sun, but that the final temperatures vary in unpredictable ways. It is to be agreed that the event of the rock's changing temperature is caused. We might say either that the sun caused it or that the sun's striking the rock caused it. It is also to be agreed that the value-fact of *T*'s being the temperature to which the rock changes in uncaused. Again, a proposition of the form 'The rock's change in temperature to T is uncaused' is ambiguous, since the expression:

(8) the rock's change in temperature to T

can mean either the same as:

(9) the-rock's-changing-temperature which event is over when the rock reaches T

or it can mean the same as:

(10) T's being the temperature the rock reaches when the event of the rock's changing temperature is over.

Under interpretation (9), (8) is an event-name, the event merely being identified by the reference to T; whereas, under interpretation (10), (8) is the name of a value-fact. The rock's change in temperature to T is caused under interpretation (9), but uncaused under interpretation (10).

On the one hand, it might be objected that if there is a fact named by (10) it is not distinct from that named by:

(11) the-rock's-change-in-temperature's going to T.

Being a *going*, the referent of (11) would seem to be an event, and, in this case, an uncaused event. But I shall not hesitate to say that events such as these are pseudo-events. For it is clear that to speak of changes going is only a picturesque way of speaking of changes being such that the things undergoing them are successively characterised by different values of certain physical functors.[1] Thus one is not to read more from (11) than can be read from:

(12) the-rock's-change-in-temperature's being a change under which the rock acquires a temperature T.

On the other hand, it might be objected that, since becoming a T-rock is, like becoming a red rock, undeniably an event, there is here an event without a cause, and not just a value-fact without a cause. In reply it must be asked how the event referred to by 'the rock's becoming a T-rock' can be uncaused and the event referred to by 'the-rock's-changing-temperature which event is over when the rock reaches T' can be caused. For, there are not two events here, since the rock is not doing two things when it becomes a T-rock and when it changes temperature. To make sense of this situation it is necessary once again to appeal to ambiguity. When the 'rock's becoming a T-rock' means the same as 'the rock's becoming something where what it becomes is a T-rock' it refers to an event, which is caused, but when it means the same as 'T's being what the rock becomes' it refers to a value-fact, which is uncaused. Similar remarks apply to 'the rock's becoming a red rock' when it is the change of colour which is caused and red's being the colour it becomes which is uncaused. In Section 5, I shall deal with the question whether the event named by the artificial expression 'the rock's *T*-changing' is caused, in light of the distinction to be drawn in Section 3 between direct and indirect causation.

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[1] Cf. Aristotle, *Physics*, 225b20-22.

The third and final example concerns the causation of the time of an event's occurrence. Suppose a jack pops out of a jack-in-the-box at irregular intervals. No cause can be found for the time at which the jack pops out. The time is then assumed to be uncaused. But whenever the jack does pop out it is not to be doubted that it is the powerful spring in the box which has this effect. So, the case is quite different from that in which not only is there no cause of the time but there is no mechanism accounting for the jack's popping out. In our example, however, it is quite true to say 'The spring caused the jack to pop out at time *t*', when, of course, one means 'The spring was the cause of the event of the jack's popping out which occurred at *t*'. But when one means 'The spring was the cause of *t*'s being the time the jack popped out', what one says is quite false.

Since at least the weak homeoetic principle is reasonable, how can this make good sense? According to this principle the cause together with certain circumstances is sufficient for the effect. But the spring together with all relevant circumstances are present all along, both when the jack does not and when he does pop out. So if there is no cause of the time of popping out there can be no cause of the event of popping out. The crucial question here is, 'Sufficient for what?' Sufficient for the jack's popping out, or sufficient for the-jack's-popping-out's occurring at *t*? With the very asking of this question the objection dissolves. Since the jack never fails to pop out sometime after having been pushed into his box, the spring-plus-circumstances are sufficient for his doing so. To argue that it is strange that the effect does not follow as soon as the sufficient conditions are present is simply to assume the point at issue, for it assumes that there can be no cause of an event without a cause, perhaps the same one, of a certain time's being the time at which that event occurs. The idea of sufficiency does not of itself imply anything about the temporal relation of cause to effect. A sufficient condition for the occurrence of the event need not be a sufficient condition for a certain time's being the time at which the event occurs. If it seems mysterious that when a sufficient condition is realised for the event's occurrence it does not occur immediately or after a set time, the mystery lies, in our case at least, in a failure of the time of the effect to be caused.

3. CAUSALITY AND MICROPHYSICS

So far, I have tried to make plausible the idea that the principle of event causality does not entail the principle of value causality. But even if the principle of event causality does not entail the principle of value causality, there may be exceptions to both. If there are exceptions to both, then it may be said that it has hardly been worth the bother to distinguish them. I shall, however, feel repaid for my efforts if an examination of typical cases of causal breakdown in quantum theory can be interpreted as exceptions to the principle of value causality but not to the principle of event causality. Whether such cases are ultimately even exceptions to the principle of value causality, that is, whether there is a subquantum level on which they are fully caused, is a question which can be ignored here.

Consider first a microphysical analogue of our ball-hitting-wall example. The precise point of impact of an electron on the fluorescent screen of a cathode ray tube cannot be accounted for within quantum theory. There is no factor within the theory which could serve as the cause of:

(13) *x*'s being the place the electron hits the tube.

Still, even though this value-fact is uncaused within the theory, the event:

(14) the electron's hitting the tube

is a caused one within the theory. That it is caused can be seen as follows. In a cathode ray tube a large electrical potential difference is maintained between the source of the electrons, the cathode, and the anode, in which there is a slit through which some of the electrons can pass to the part of the tube containing the fluorescent screen. The electrical field between the cathode and the anode accelerates the electrons in the direction of the anode. Thus it can be said that event (14) is caused by the electrical field. Though there is no cause of *x*'s rather than *y*'s being the place the electron hits the end of the tube, there is a cause of the electron's hitting the end of the tube rather than, say, reversing its direction when it reaches the slit.[1]

Since an accelerated electron may hit the anode and hence not pass through the slit or, if it does pass through, may be trapped by an ion before reaching the end of the tube, how can it be said that a given impact at the end of the tube is caused by the field? The objection becomes even stronger when it is noted that whether the electron will encounter an obstacle along the way is a matter subject to the same indeterminacy noted in connection with the point of impact. This objection rests on the weak homeoetic principle. The electrons which do and those which do not reach the end of the tube are accelerated by the field under similar circumstances. But to answer this objection it is necessary to note that to be sound the weak homeoetic principle must include under the requirement of similar relevant circumstances the requirement of non-interference with the process initiated by the cause. In the case of the point of impact, it is not obstacles which keep *x* from being the point of impact of all the electrons accelerated by the same field and passed through the same slit. But though all electrons accelerated by the field do not hit the screen, it can still be said that the impacts which do occur are caused by the field, since interference accounts for the impacts which do not occur. However, one might say the objection could be overcome simply by rejecting the weak homeoetic principle on the ground that for few if any true singular causal claims does anyone know all the specific relevant circumstances. But this ground is irrelevant since the principle requires neither that anyone know nor that causal claims entail sets of specific circumstances. The principle is warranted by and given its meaning by the rule that belief in the claim that this *A* caused this *B* is justified only if the occurrence of another *A* without a *B* is accounted for through a difference in circumstances which could be shown empirically to be a relevant difference.

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[1] Cf. M. Bunge, *Causality*, Meridian, New York, 1963, p. 15. It was Bunge's aperçu—that, in his example of the scattering experiment, the oncoming particle's deflection is caused though the precise angle of its deflection cannot be predicted—which provided a decisive stimulus to my thought on the causal principle. For the same insight, see W. Heisenberg, *Physics and Philosophy*, Harper, New York, 1962, p. 89; and D. Bohm, *Quantum Theory*, Prentice-Hall, New York, 1951, p. 150.

For a microphysical analogue of our jack-in-the-box example, consider the phenomenon of nuclear decay. The emission of an alpha particle from a uranium nucleus is a caused event, whereas the time of this alpha-particle emission is, within quantum theory, an uncaused value-fact. Within quantum theory, '*nothing* determines this time. It is supposed to be completely arbitrary, and not capable of *ever* being related to anything else by means of any kind of laws at all.'[1] Nonetheless, within quantum theory it is inescapable that the emission itself is caused, for the nucleus is not like a jack-in-the-box without a spring in it. In the nucleus, there is, in addition to the so-called strong interaction which binds the nucleons together, an electrostatic force of repulsion between the protons. The importance of the electrical repulsive force is clear from the fact that mirror nuclei, that is, nuclei differing from one another only by an interchange of protons and neutrons, have greater or lesser binding energies according as they have a smaller or a greater number of protons. Another indication of the disruptive force of the protons' positive charges is that uranium is the largest naturally occurring nucleus. For, the electrical forces, which are effective at greater distances than the nuclear forces, will have a more marked effect in the larger nuclei. Alpha-particle decay is, however, not a simple matter of the alpha particle's acquiring, through the electrical repulsive forces, an energy equal to the potential energy it would acquire in crossing the barrier between it and the region outside the nucleus. In fact, the particle 'tunnels' through the barrier, in a manner predictable statistically by quantum theory, not having an energy equal to the potential energy of the barrier.[1] Still, it is the electrical forces in the nucleus which cause the nucleus to decay.

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[1] D. Bohm, *Causality and Chance in Modern Physics*, p. 88, n. 1.

So long as I stay with cases of these types I seem to be able to show that putative exceptions to the principle of value causality are not exceptions to the principle of event causality. But there are cases of another type which seem to be direct exceptions to the principle of event causality. Consider a crystal with the following properties. It absorbs all light plane polarised in a certain direction. It transmits all light which is plane polarised at right angles to that direction. And it transmits a fraction $\sin^2 a$ of light which is plane polarised at an angle a to the same direction. Now treat the light as composed of photons. Some photons of the obliquely polarised light will be transmitted and the others will be absorbed. In this case the probability of transmission is given by $\sin^2 a$. For an individual photon, neither absorption nor transmission is predictable.[2] It would then seem that we have here not just uncaused value-facts but uncaused events, for the transmissions or absorptions of the individual photons of the obliquely polarised light are not caused.

Up to now the basic strategy has been to change prepositional phrases—'at x', 'to T', 'at t'—into relative clauses—'which occurred at x', 'which is over when the rock reaches T', 'which occurred at t'. With this strategy we arrive at the position that event-prepositionals—those details of events expressed by prepositional phrases in their names—can be uncaused while the events themselves are caused. But when event names do not have prepositional components this strategy is no longer helpful. When, for example, an event is referred to simply as the absorption, there is no prepositional phrase to be turned into a relative clause. Even if we refer to it as the absorption of a photon, separating off the consideration of the photon from that of the absorption is not helpful here, since it is not the photon's being the absorbed entity which is uncaused but the absorption itself. There is, however, a new strategy which will enable us to advance on this front as well.

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[1] Cf. R. B. Leighton, *Principles of Modern Physics*, McGraw-Hill, New York, 1959, pp. 524-9.

[2] Cf. P. A. M. Dirac, *The Principles of Quantum Mechanics*, 3rd ed., Clarendon Press, Oxford, 1947, p. 6.

Suppose I call a meeting of certain individuals and that in response to my call they do come together. I could not have anticipated they would fight on coming together, but they do fight, and so the meeting I called is a fight. Now, in a certain sense I brought about the fight; I brought about the meeting which was a fight. But in another sense I did not bring about the fight; the meeting I brought about only happened, as far as my agency was concerned, to be a fight. On the other hand, if an event does not, as far as my agency is concerned, just happen to be a fight, but if I actually provoke a fight, I am the cause of fighting's being the character of the event.

When 'I caused the fight' has the support of 'I caused the event and fighting's being its character', the expression:

(15) the fight

is replaceable in 'I caused the fight' by:

(16) the event as a fight.

When, however, 'I caused the fight' has the support only of 'I caused the event and fighting was its character', (15) is replaceable by:

(17) the event which happened to be a fight.

Finally, when there is the support of 'I caused fighting's being the event's character' but not of 'I caused the event', (15) is replaceable, not by (16), but by:

(18) fighting's being the character of the event.

It will be helpful to have a nomenclature for recording these distinctions. When in 'I caused the fight', 'the fight' is replaceable by (16), I shall speak of my causing the fight as *direct and proper* event causation; when 'the fight' is replaceable by (17), I shall speak of my causing the fight as *indirect but proper* event causation[1]; and, finally, when 'the fight' is replaceable only by (18), I shall speak of my causing the fight as *improper* event causation, since it is actually value-fact causation.

It is obviously indirect but proper event causation to which I shall appeal to get around the photon absorption objection. But first, further clarification of a general nature. (*a*) Indirect but proper event causation rests on direct and proper event causation. If I cause the fight indirectly, then there is some event, which happens to be a fight, which I cause directly. This event can be referred to with greater or lesser specificity. I can speak of it more specifically as the meeting which happens to be a fight or less specifically as the event which happens to be a fight. Assume that the common nouns applying to such an event form a discrete series going from greater to lesser specificity. There may be a common noun such that it is the most specific in the series which as applied to the event allows us to say the event is directly caused. The event as referred to with this noun is to be called *the specific* direct effect. The event as referred to by more specific common nouns in the series is indirectly caused, and as referred to by less specific nouns it is to be called *a generic* direct effect. This notion of the specific direct effect is admittedly artificial. But no important questions are begged by using it to interpret the following question. (*b*) Can it be the case that my causing the fight, the meeting, the motion of men, . . . are all instances of indirect but proper event causation, whereas only my causing the event is an instance of direct and proper event causation? In other words, can the event as such be the specific direct effect? This is not to ask whether an effect can be an event which belongs to no kind and occurs at no time and no place. The conceptual impossibility of such a substratum-type event's existing as a distinct thing makes it absurd to raise the question as to whether such an event could be an effect. The question I am raising is whether a cause could cause a fullblooded event just as an event without causing it as a motion of men, as a meeting, as a fight, or as occurring at place *x*. There seems to be no reason for drawing the line between what can and what cannot be the specific direct effect at one place rather than at another in the series. If in one case a meeting is the specific effect and a motion of men is a generic effect, then there is no reason why in another case another meeting cannot be only an indirect effect whereas it is a motion of men which is the specific direct effect. Likewise, if in one case a motion of men is the specific effect, there is no reason why in another case another motion of men cannot be only an indirect effect whereas it is an event which is the specific direct effect. Thus, since in general it is reasonable to say that an *A* might be the specific effect in one case even though in another case the *A* is only a generic effect, it should be reasonable to say in particular that an event might be the specific effect in one case even though in another case the event is only a generic effect. There is in principle no difference between events and events referred to in definite ways as regards the possibility of being specific effects.

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[1] Cf. Aristotle, *Physics*, 224a31-33.

Let it be granted that there is no direct cause of the absorption of the photon. If the absorption is caused, it is only indirectly caused. But for it to be indirectly caused there must be a cause which has as its specific direct effect either the event or the event as referred to by some expression more specific than 'event' and more general than 'absorption'. Such a cause can be easily identified; it is the source of the photon. If, as we shall assume, the light passes from the source through an aperture to the crystal, then in these conditions the source causes photonic events at the crystal, whether the further features of these events are caused or not. Since the event as a photonic event at the crystal is caused and, hence, since the photonic event at the crystal is directly caused, we need not resort to the event as the specific direct effect. Whatever the specific direct effect is, it is less general in its description than the event itself. If the arrangement were such as to prevent reflection altogether, the specific direct effect might be the nonreflecting photonic event at the crystal.

The principle of event causality is not violated in the photon example for the reason that, though there is no direct cause of its absorption, if it is absorbed, or of its transmission, if it is transmitted, there is a direct cause of the photonic event at the crystal. Since indirect causation at one level implies direct causation at a level of greater generality, the principle of event causality can be formulated either as the principle that every event no matter how specifically referred to has at least an indirect cause or, equivalently, as the principle that every event if referred to in a sufficiently generic fashion has a direct cause. If the photon had had no source—had either been associated with light coming from infinity or had inexplicably popped into existence sometime before meeting the crystal—, then, provided that there was no force deflecting the photon into the direction of the crystal, the principle of event causality would have been violated. In this hypothetical case the absorption would have been a spontaneous event, that is, an event which lacks even an indirect cause.

4. RELATIONS BETWEEN THE VARIOUS CAUSAL PRINCIPLES

By way of summarising the results thus far established and at the same time pointing out the nature of some of the remaining problems, I shall consider the logical relations between the principle of event causality, of value causality, and of prediction. (*a*) 'If the principle of event causality then the principle of value causality.' This proposition has been discredited by a consideration of the above examples, hypothetical and real. The main thesis of this paper—that, though quantum theory may offer exceptions to the principle of value causality, it provides no exceptions to the principle of event causality—depends on this proposition's being false. (*b*) 'If the principle of value causality then the principle of event causality.' Could all the value-facts associated with an event be caused and the event itself still be uncaused? Of course it is possible for a cause to be the cause of a value-fact associated with an event without being the cause of the event, or in the vocabulary of Section 3, while being only an improper cause of the event. If x lights a Bunsen burner and y makes the flame violet by putting a potassium compound in it, then y is the cause of violet's being the colour of the flame even though only x is the cause of the flaming. But since in every case there is the value-fact of eventhood's being a character of the thing in question, it is trivially true that if every value-fact is caused the event is caused. So the interesting question is whether the event is caused if every noneventhood value-fact is caused. I wish to leave this question open, remarking only that now I see no compelling reason for answering it affirmatively.

(*c*) 'If the principle of prediction then the principle of value causality.' This proposition is true only if a sufficient condition for a value-fact's having a cause is that the possession of the value could be predicted from previous conditions by the laws of nature. I have tried to show elsewhere[1] that this is not a sufficient condition since there are numerous cases in which action, in a sense irreducible to law-governed sequence, is required for causation and since there are in fact cases of law-governed sequence which fail to be cases of causation because of the absence of the required action. The notion of action in question is not limited in its application to human and other animate causes. I have, in the present paper, contrasted the principle of event causality with the principle of value causality rather than with the principle of prediction, which is more common among scientific philosophers, in order to separate off the issue as to whether causation typically involves action rather than merely law-governed sequence. Nonetheless, it is obvious that I have always looked for dynamic features of a given situation in order to find a cause. Thus my classification, at the end of Section 3, of the absorption of a photon coming undisturbed from infinity as a spontaneous event is one I would have no reason to abandon even if the path of the photon were governed by a deterministic law.

(*d*) 'If the principle of value causality then the principle of prediction. For this proposition to be true it must be the case that the weak homeoetic principle is true as applied to value-fact causation. That is, it must be true that every singular causal claim of the form 'This x is the cause of this value-fact y' implies a corresponding proposition of the form 'There is a set of circumstances z, which is present with this x, such that any x in z will be accompanied by a value-fact y'. Here the requirement of similar circumstances is understood to include not just a requirement of similar conditions surrounding the cause but also one of the absence of interference with a process, if there is one, intermediate between the cause and the effect. In this form the weak homeoetic principle can, I believe, be defended against the usual objections to it. Its force is only to exclude the possibility of a 'unique' effect, that is, of something which could justifiably be claimed to be the effect of a certain cause in certain circumstances even when no attempt is made to explain why, in known cases, it does not result from a similar cause in similar circumstances. The truth of the weak homeoetic principle is sufficient to support the implication from the principle of value causality to the principle of prediction only if the regularities required by the weak homeoetic principle are either themselves laws or follow from laws, since the principle of prediction is stated in terms of laws. But the regularities required by the weak homeoetic principle will in general be too complex to count as laws. Whether they follow from laws, and hence whether the principle of value causality implies the principle of prediction, is a question I shall leave open.

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[1] M. Fisk, 'Causation and Action', *The Review of Metaphysics*, 19 (1965), pp. 235-47.

The weak homeoetic principle as applied to event, as opposed to value-fact, causality is more complex. It is to be stated as follows: Every singular causal claim of the form 'This x causes this event y' implies a corresponding proposition of the form 'There is a set of circumstances z, which is present with this x, and there is an event-kind y' such that this y is a y' and such that any x in z will be accompanied by a y''. The need for 'y'' as well as for 'y' in this formulation is due to the fact that this y may only be indirectly caused by this x. Thus to find an invariable accompaniment for an x one must consider it as a y' and not as a y. For stronger claims of the form 'This x causes this event y directly' it is unnecessary to employ 'y''.

From considerations of the sort already introduced, it can be seen that both of the remaining propositions—(*e*) 'If the principle of event causality then the principle of prediction' and (*f*) 'If the principle of prediction then the principle of event causality'—are false.

It is to be noted that there is a certain parallel between Bohr's principle of complementarity and the thesis that there can be event causality without value-fact causality.[1] In quantum theory one cannot give both a precise space-time description of a system and a precise energy-momentum description of it. But causality in the sense of control by deterministic laws is concerned with the energy-momentum description, for which the deterministic conservation law holds. Thus, if space-time and energy-momentum are complementary, then space-time and causality in this sense are complementary. In like manner, if one sacrifices the consideration of value-facts, one can have a causal principle, the principle of event causality. But this is no more than a superficial parallel; the truth of each view is logically independent of that of the other. On the one hand, energy-momentum value-facts could be among those which fail to be governed by deterministic laws in spatio-temporally fuzzy systems without implying that there are uncaused events. On the other hand, since for the principle of event causality, causality is not just the control of law, it is conceivable that the conservation of energy should hold even though there are, in the sense given at the end of Section 3, spontaneous events. If, to avoid the objection concerning photon absorption, we had appealed to the principle of complementarity and, thus, exchanged the particle for the wave picture of the processes, then it would have been impossible to claim this independence. As it is, we found that remaining within the particle picture did not exclude event causality.

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[1] N. Bohr, *Atomic Theory and the Description of Nature*, Cambridge University Press, Cambridge, England, 1934, p. 60. Cf. also N. Bohr, 'Discussion With Einstein on Epistemological Problems in Atomic Physics', in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp, Tudor, New York, 1949, pp. 210–11.

5. CAPACITIES AND CAUSALITY

It has been my concern only to defend the principle of event causality against objections and not to justify it by positive arguments. But I wish to conclude by saying something about the possibility of justifying the principle of event causality through appeal to a principle about the actualisation of capacities. Events and values occur or obtain as actualisations of capacities. How are the capacities actualised? One answer is the principle of actualisation: Capacities are not actualised without causes affecting things with these capacities. It is to be noted that the principle of actualisation does not require a cause of the realisation of each capacity. The principle of actualisation is, for example, satisfied by our hypothetical ball since though there is no cause realising its capacity to be at *x* on the wall there is a cause realising its capacity to hit the wall. The principle of actualisation, like the principle of event causality, may hold even though the principle of value causality fails.

Still, the principle of actualisation succeeds in expressing the demand that, since capacities are different from their actualisations, something must intervene in regard to each one of them if it is to be actualised. For, that which intervenes need not be the cause of the given capacity's actualisation; it is sufficient that it be the "occasion" of its actualisation. The sun is the occasion of *T*'s being the final temperature of the rock if and only if (*a*) it does not cause this value-fact but (*b*) it does cause the rock's changing temperature. Causes of events are, then, occasions of the associated value-facts when they are not causes of these value-facts. Likewise, causes of the actualisations of capacities to enter into events are occasions of the actualisations of certain of the capacities for values associated with those events. Since causes can then be occasions, the principle of actualisation is sufficient to guarantee that all capacities are actualised by the intervention of something actual, without at the same time guaranteeing that the actualisation of every capacity for a value is caused.

Occasions correspond to indirect but proper event causes. The sun is the *occasion of the value-fact* of *T*'s being the rock's final temperature, since in our example it does not cause this value-fact. Suppose 'the rock's *T*-changing' means the same as neither (9) nor (10). Rather it names an event in respect to which *T* is an essential internal feature. Now the sun is not the direct cause of the rock's *T*-changing. But since it is the direct cause of an event which happens to be a *T*-change, the sun is the *indirect cause of the event* of the *T*-changing.

It is reasonable to suppose that the principle of actualisation implies the principle of event causality. And it might be maintained that the principle of actualisation is not only obviously true but also an obvious analytic truth. For, the only alternative to the principle of actualisation seems to be the view that capacities realise themselves. This alternative is surely ruled out on the ground that capacities are not actual things and only actual things can be causes and occasions. The principle of actualisation then seems to provide a way of showing not just that the principle of event causality is true but also that it is analytic.

True, it would be conceptually absurd to say that a capacity realises itself. But this can be the sole alternative to denying the principle of actualisation only if one has already accepted the idea that a cause must be involved in the transition from capacity to realisation. If one has not already accepted this idea there is no difficulty in allowing for the possibility that capacities should become actual without any cause, even themselves. But accepting this idea—that actualisations must be caused—will be as hard for someone who is troubled by the principle of event causality as accepting the principle of event causality itself. Thus pushing back to capacities will not make it clear that the principle of event causality is analytic, if it was not clear that it was analytic at the outset. Here I mean analytic in the broad sense of being true by virtue of the meanings of terms, rather than in the narrower sense of leading formally to a contradiction if denied.

It seems highly improbable that the principle of event causality is analytic and hence all the more improbable that the principle of value causality is analytic. There is nothing amounting to a violation of rules of usage in concluding from experiment and theory that the place of a particle's impact is uncaused or even that the event of the impact is spontaneous. A person who draws such a conclusion does not sever his relations with the scientific community; his conclusion can be tested, and if one disagrees with it one can attempt to refute it with experiment and theory.[1] It would be absurd to say that because testing it is a search for causes science is committed to universal causality; if it were not absurd then it would have to be reasonable to say that science is committed to the existence of what might falsify each of its hypotheses. Nor can the analytic character of the principle of event causality or of value causality be shown by claiming that a basic rule of language is that language must always be improved in its explanatory resources.[2] Such a rule expresses only an ideal, and not being able to follow it would be no reason for saying either that we are no longer language users or that we misuse the terms of language. It would be helpful if attention were turned away from the question whether the principle of event causality and of value causality are analytic or even necessary until there has been a detailed investigation of whether there *could be* no caused events and no caused value-facts. At least we know some events and some value-facts *are* caused. But an inquiry into whether all events or all value-facts *must be* caused does not rest on the knowledge that all events or all value-facts *are* caused.

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[1] Cf. K. Popper, *The Logic of Scientific Discovery*, Hutchinson, London, 1959, pp. 248–9.

[2] Cf. W. Sellars, 'Counterfactuals, Dispositions, and the Causal Modalities', in *Minnesota Studies in the Philosophy of Science*, vol. II, ed. H. Feigl, M. Scriven, G. Maxwell, University of Minnesota Press, Minneapolis, 1958, pp. 306–7.

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Types of Inter-Theoretic Reduction*

by LAWRENCE SKLAR

I

The history of scientific theorising presents us with a number of notable accomplishments conveniently grouped together by methodologists as cases of 'the reduction of one theory to another'. The members of this family are, however, by no means identical twins. That is, despite similarities among all the various cases of reduction, a number of distinctive sub-types, whose members all resemble each other far more than they do any members of other sub-types, can be discerned. My aim is to provide the beginnings of a 'taxonomy' of inter-theoretical reductions. This will, of necessity, involve the examination of a number of extant theories of reduction, and, in some cases, their rejection or modification.

The philosopher's interest in reduction in the natural sciences is often motivated to a large extent by the hope that a sufficient understanding of reductions of this sort will provide insight into traditionally philosophical problems, such as the mind-body problem and the complex of issues concerning phenomenalism. I shall avoid concerning myself with these philosophical issues as much as possible, only allowing myself a few remarks on the possible relevance of the results obtained in the body of the paper to current philosophical speculation at the end of my more-or-less 'descriptive' methodological survey.

I also wish to avoid the complex of issues in the foundations of mathematics often labelled problems of reduction. Whatever is meant by the claim that arithmetic is reducible to logic, or more conservatively to set-theory, it is not that arithmetic is related to the more fundamental theory in anything like the way in which a physical theory is related to a theory to which it reduces.[1]

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* Received 11.ii.66

[1] For a discussion of the nature of reduction in mathematics, cf. N. Goodman, *The Structure of Appearance*, Cambridge, Mass., 1951, ch. I and P. Benacerraf, 'What Numbers Could Not Be', *Phil. Rev.* 74, (1965) 47-73. It is Goodman's contention, in the reference cited, that the type of reduction appropriate for mathematics is adequate as an account of much philosophical reduction as well.

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In what follows we shall be concerned with explicating the relations which hold between two theories, T1 and T2, when the latter is said to have been reduced to the former. As a matter of convenience I shall often deal with the expressions of the theories in some natural language rather than with the 'quasi-linguistic' theories themselves, thus identifying theories with their expressions, theoretical concepts with descriptive general terms, etc. While risky in the context of some discussions, this deliberate ambiguity will probably not lead to difficulties in the problems we will be considering.

We may begin our taxonomy with a division of reductions into those cases designated by Nagel 'homogeneous' and those designated 'inhomogeneous'. Two theories will be said to be *homogeneous* if they share the same conceptual apparatus, and *inhomogeneous* if one contains a concept not found in the other. Actually we shall only be concerned to distinguish those cases in which the concepts of the reduced theory are a subset of those of the reducing theory (*homogeneous reduction*) from those cases in which the reduced theory contains some concept not present in the reducing (*inhomogeneous reduction*).

A particularly simple case of homogeneous reduction is that in which the reduced theory is simply deducible from the reducing theory. In such a case the problem of reduction presents no methodological difficulties not already encountered in an attempt to understand the 'internal' structure of a theory. Indeed, why call the relationship a reduction at all, where the reduced theory is, properly speaking, only a fragment of the reducing, developable from it by mere deductive reasoning? Had the reducing theory been developed first, and the reduced subsequently derived from it by a chain of deductive inference, we would not be tempted to describe the relationship between the theories as one of reduction. Where, however, the 'smaller' theory is developed first, and only later shown to be a consequence of a 'larger', newer theory, we are inclined to speak of a reduction having taken place, since the reduced theory had its independent existence prior to either the development of the reducing theory or to the demonstration of the deductive relationship between the two theories.

Matters are not, however, quite this simple even in these very rudimentary cases of reduction. If one looks for examples of reduction from the history of science, strictly derivational reductions are few and far between. One can *construe* various relationships of a strictly derivational sort as reductions, but an examination of cases of reduction pre-analytically so-called, shows that even in the case of homogeneous theories reduction is very rarely derivation.

Consider, for example, the reduction of the Galilean laws of falling bodies or the Keplerian laws of planetary motion to Newtonian mechanics. As has been often pointed out, the reduced theory cannot be derived from the reducing, despite the fact that the concepts of the reduced are certainly a subset of the concepts of the larger theory. What can be derived from the reducing theory is an *approximation* to the reduced, where this notion of approximation is suitably relativised to a degree of accuracy, a range of values of the independent parameters, etc. One might think that the modification of the account of reduction from the strictly to the approximately derivational is a matter of no great philosophical interest, although it might be a matter of some complexity; we all know what 'approximately' means, but just try to *say* what it means for the general case. But certain recent remarks by philosophers about reduction show that somewhat more needs to be done to clarify the matter of approximate derivational reduction.

Consider, for example, the following argument: If T2 reduces to T1, then T1 explains T2. But suppose that the laws deducible from T1 are at best approximations to the laws of T2. To say that A is at best an approximation to B is to say that A and B differ, and that they are, in fact, incompatible. But now consider the so-called deductive model of scientific explanation. According to this account it is a necessary condition of one theory's explaining another that the latter can be deduced from the former. But since one theory can explain another incompatible with a theory deducible from the explanans theory, we can see that the deductive account of explanation cannot possibly be correct.[1]

What are we to say to such an argument? Perhaps the following is a useful approach. First, it is undoubtedly true that in some cases of reduction, pre-analytically so-called, the reduced theory is incompatible with the reducing. Approximate derivational reductions are of this sort and, as we shall see, so are reductions of a rather different kind. A fundamental distinction appears to be necessary between reductions in which the reduced theory is *retained* as correct subsequent to the reduction, or even better confirmed by its being reducible to a well confirmed more general theory, and those reductions in which the reduced theory is instead replaced by the theory to which it reduces. The difference between these two cases is so radical that it might be better to restrict the extension of the term 'reduction' so as to exclude replacements as cases of reduction. Two considerations militate against this move, however, useful as it would be in eliminating possible confusion. First, many important cases of reduction in science, pre-analytically so-called, are clearly replacement-reductions. Second, there are many important ways in which replacement-reductions are like those reductions in which the reduced theory is retained, despite the obvious and important differences between the two kinds of reduction.

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[1] P. K. Feyerabend, 'Explanation, Reduction and Empiricism', in H. Feigl and G. Maxwell, (eds.), *Minnesota Studies in the Philosophy of Science*, 3, 43-52.

Although I think some recent authors have gone too far in claiming that all, or at least all significant, reductions are replacement-reductions,[1] I do not want to minimise the importance in science of such 'discarding' replacements in which the reduced theory is shown to be incorrect by the very act of its being reduced. But the conclusions to be drawn from this observation must be examined with care. Consider, for example, the use of the existence of replacement-reductions to attack the deductive theory of explanation. Does this attack hold up under scrutiny? I think not. But this is not easy to demonstrate, at least to everyone's satisfaction. For the issue is as to the valid application of the term 'explanation', and such *decisions* regarding usage are hardly amenable to deductive proof. What can be provided is a clarification of the point at issue and some justifications for a *recommendation* to apply the term 'explanation' in a particular way.

At least one plausible distinction to be drawn is that between one theory's *explaining* another, and the former's *explaining away* the latter. Is it plausible, for example, to maintain that quantum mechanics, assumed for the moment to be correct, *explains* an admittedly incorrect Newtonian mechanics? Surely not, for an incorrect theory cannot be explained on any grounds. ('Explain why you were late.' 'I wasn't.' 'I am well aware of that fact but I insist upon an explanation for your lateness anyway.') If we extend the extension of the term 'explanation' in scientific contexts, so as to allow the possibility of explaining incorrect theories we are forced by 'parity of discourse' to allow discourse in our ordinary language which we would intuitively regard as senseless.

What, then, *does* quantum mechanics explain, if it fails to explain Newtonian mechanics? Among other things it explains why Newtonian mechanics *seemed* to be correct; why it met with such apparent success for such a long period of time and under such thorough experimental scrutiny. ('Explain why you were late.' 'But I wasn't.' 'Ah! Now I see you are right. You weren't late. But why did everybody think you were?') If the distinction between explaining a theory and explaining its apparent success is drawn there is no reason whatever why we cannot admit that T2 is reducible to T1, admit that T2 is incompatible with T1, and yet deny that T1 explains T2. Granting that many reductions are radical replacements of the reduced theory by the reducing one need not in any way loose one's confidence in a strictly deductive account of explanation.

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[1] See, for example, T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962, *passim*.

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The problem of reduction becomes more difficult when the theory to be reduced contains concepts not present in the reducing theory. One solution is simply to dispose of the reduced theory entirely, for a non-existent theory presents no difficulties of un-accounted-for concepts. But this radical course seems inappropriate in many cases. Often we feel inclined to accept the retention in some sense of the reduced theory, despite the fact that it contains concepts not present in the reducing, nor 'definable away' in terms of the concepts of the latter theory. In one type of degenerate case it will be possible to strictly deduce the reduced theory from the reducing, despite the presence in the former of 'obtrusive' concepts. We can, for example, deduce from a theory of the form 'p' a theory of the form 'p or q', even if 'q' contains terms not present in 'p'. But such 'trivialized' deductions will hardly do justice to the many cases of significant reduction in science in which the reduced theory contains a concept not present in the reducing.

Such inhomogeneous reductions are common. The theory of heredity speaks of genes, while this concept appears nowhere in the chemical side of biochemistry. Physical optics refers to light-waves, whereas the electromagnetic theory to which it reduces refers only to electric and magnetic fields, etc. How can it be possible for one theory to be reducible to another when it is patently clear that the sentences of the former could not be deduced, or even approximately deduced, from those of the latter, even allowing for linguistic transformations such as substitutions of synonymous terms and grammatical transformations? We shall see that at least two kinds of inhomogeneous reduction are possible: a weaker variety in which the retention of the reduced theory is retention only in a much weakened sense; and a stronger form in which the reduced theory remains well established even after the reduction, and may even be given added confirmation by it.

A model of reduction proposed as a *general* account of reduction by Kemeny and Oppenheim will provide us with some insight into the weaker version of inhomogeneous reduction.[1] Their theory, of an obviously positivistic and instrumentalistic nature, begins with a familiar division of the vocabulary of a theory into an observational and theoretical part.

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[1] J. Kemeny and P. Oppenheim, 'On Reduction', *Phil. Studies*, 7, (1952), 6-19.

Derivative from this is a division of the sentences of the theory into observational—those having as their only essentially appearing non-logical terms observation terms—and theoretical—those containing at least one essential occurrence of a term from the theoretical vocabulary. Reasoning that the sole purpose of a theory is to provide observational consequences, one can then define an equivalence relation between theories by the notion that two theories are equivalent if the same class of observational sentences is derivable from each. But equivalence relations are symmetrical and reductive relations asymmetrical! The asymmetry is introduced through the notion of the *systematic power* of a theory. This is a measure of the ability of the theory to predict as wide a range of phenomena as possible from as little data as possible; and, although there exists no clear explication which is intuitively entirely satisfactory, interesting attempts in a direction toward it have been made.[1] Clearly if two theories are predictively equivalent and one is of greater systematic power than the other, then the more powerful theory is preferred as reducing theory. Again, if the class of deducible observational consequences of one theory is a proper subset of the consequences of another of greater or equal systematic power, then the more general theory is preferred. By means of these notions we can introduce an asymmetric relation of reduction on theories as follows: T2 is reducible to T1 if (*a*) all the observational sentences deducible from T2 are deducible from T1 as well, and (*b*) T1 is of greater systematic power than T2. The harmony of this account of reduction with the more general thesis that theories are 'instruments for the correlation of observations' is clear.

Three reasons militate against the acceptance of the Kemeny-Oppenheim account as a general analysis of reductions in science. Two of these are objections to the general analysis of the nature of theory presupposed by their account. As a consequence I shall be able only to state them rather than to argue for them. First, the distinction between observational and theoretical terms (these latter would be more appropriately designated 'non-observational') is one which has been much doubted in recent years. Without resorting to the stipulation of observational terms as terms in a phenomenal 'sense-datum' language, an epistemological radicalism now shunned by most philosophers of science, it seems difficult to provide anything like a division of the world into observables and non-observables. Even if we could make such an epistemological distinction it is not at all certain that a 'map' of it in terms of a linguistic distinction of kinds of *vocabulary* could be drawn.

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[1] C. Hempel and P. Oppenheim, 'The Logic of Explanation', *Phil. of Science*, **15**, (1948), 164, and J. Kemeny and P. Oppenheim, 'Systematic Power', *Phil. of Science*, (**22**, (1955), 27-35.

Second, the 'positivistic-instrumentalistic' view of the role of theories in science clearly suggested and presupposed by the Kemeny-Oppenheim account is itself a view of physical theories hard to defend. But without such a positivistic epistemology and metaphysics it is hard to see the plausibility of the Kemeny-Oppenheim account as an explication of the meaning of reduction in natural science.

Lastly, some empirical examination of science casts even more doubt upon this account of reduction. It is hard to find in the history of science any examples of reduction, intuitively so-called, which satisfy the account; or, more correctly, it is hard to find any examples which satisfy the Kemeny-Oppenheim account without satisfying the much more demanding accounts of strict derivational reduction or of the more rigid kind of inhomogeneous reduction we shall examine later as strong inhomogeneous reduction. That is, the model seems too *weak* to account for some types of reduction, failing to illuminate the strength of the connection between reduced and reducing theory; and too *strong* for the remaining cases, making demands upon the relation between reduced and reducing theories which are simply not satisfied.

To see the *weakness* of the model for some cases we need to explicate the stronger connection holding between the related theories. We have done this in the case of strict derivational reduction and shall proceed to the strict inhomogeneous reductions later on. To see the other side of the coin we need only note that the very cases the Kemeny-Oppenheim account was introduced to handle fail to meet its requirements. The account was originally introduced to provide for those cases of reduction in which the reduced theory was, in fact, replaced by the reducing. The authors seemed to have hoped that the incompatibility of the reduced and reducing theories in these cases could be isolated at the theoretical level, the level at which on their account no relation of equivalence or inclusion between the theories is required. But this is not true. For if the reducing and reduced theories made *no* difference in our observational predictions, in so far as they 'covered' the same type of phenomena, then what would have motivated us to *discard* the reduced theory in favour of the reducing? No matter how stringent one's requirements for a state of affairs to be 'observable' are, for example, Newtonian mechanics and special relativity simply do not predict the same observational results, or even *approximately* the same observational results, at least for a significant and easily specifiable range of experimental conditions.

To summarise, the Kemeny-Oppenheim account fails to hold for any reductions except those satisfying much more stringent and illuminating conditions. In other words, the only reductions which hold between theories making the same observational predictions within the range of phenomena 'covered' by the reduced theory, are reductions which display an important relationship between the *theoretical* parts of the reduced and reducing theory.

An account of reduction derived from the Kemeny-Oppenheim account does, however, seem to do justice to the weak inhomogeneous reductions they intended to explicate. Let us suppose we are presented with two theories, the one, T2, to be reduced to the other, T1. In addition suppose that T2 contains some concepts not present in T1. How then can T2 be reducible to T1? Suppose that of the concepts contained in T2 a certain proper subset consists of concepts shared with T1. Let us call these concepts the *external* concepts of T2 and T1, designating the remaining concepts the concepts *internal* to their theories. This distinction is one to be relativised to the particular reduction in question and not to be identified with any variety of observational/theoretical distinction. The external concepts may be either observational or theoretical concepts, no matter how this latter distinction is drawn. Further, suppose we consider the sentences deducible from T2 and from T1 containing as essentially occurring descriptive terms only terms of the external sort. We call these the *external sentences* of the respective theories. In addition, suppose that within the joint domain of external sentences of the two theories there is a subset of sentences derivable both from T1 and from T2, or derivable from T1 and such that approximations to them are derivable from T2. Where these conditions are satisfied we shall say that T2 is *partially* (or *approximately partially*) *reducible* to T1.

Examples of partial reduction are easy to come by. If we first restrict our attention to sentences framable in purely kinematic concepts, and then further restrict our attention to the subset of sentences in this class dealing with sufficiently low velocities, we find for every sentence in this subset derivable from the relativistic theory there is a sentence approximative to it derivable from the Newtonian theory. It is only in this extremely weak sense that Newtonian mechanics is reducible to special relativity. We should note that a theory may be partially reducible to any number of other theories mutually incompatible both with regard to their observational consequences and with regard to their theoretical conceptual apparatus.

I might remark in passing that at least one philosopher would find fault in my maintaining that the replaced theory has any conceptual apparatus at all in common with the theory which supplants it.[1] While admitting the force of this argument, partially based upon the notion that the meaning of a concept is based upon its role within the total theory in which it occurs and partially upon the radical 'conceptual shifts' necessary to go from, say, Newtonian space and time to relativistic space-time, I doubt its validity. But since I am not prepared to offer anything like even the beginnings of an analysis of the notion of the meaning of a theoretical term, I can only state my prejudice and admit my unease.

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[1] Cf. P. K. Feyerabend, op. cit. section 7.

It is only reasonable that an account of reduction derived from an instrumentalistic view of the nature of theories is applicable in some cases. For what is it that makes us want to speak of some theoretical replacements as reductions—say the reductions of Newtonian mechanics to relativity theory and to quantum mechanics, or the reduction of hydrodynamics, with its presupposed continuous fluid media, to the atomic theory of fluids; and to speak of other replacements—say of the phlogiston by the oxygen of combustion or the caloric by the energetic theory of heat—as the mere discarding of one theory in favour of a better? It is only the survival of the older theory, in the reductive cases, as a 'useful instrument for prediction', despite its known falsity as a scientific theory. If reference to caloric, its presence and absence, its rate of flow, the capacity of bodies to absorb it, etc., were as useful to engineers—despite the fact that there is no caloric—as is the hydrodynamic theory to boat designers—despite the fact that there are no microscopically continuous fluid media—there is little doubt that we would speak of the reduction of the caloric theory to the energetic, rather than welcome its overthrow as we are now inclined to do. The relation of partial reduction is a very weak one and must be, as noted, relativised to many factors idiosyncratic to the particular reduction in question. But why not? After all, the theory reduced is not only incorrect but conceptually untenable as well. The only thing good about it is that it is a useful device for making predictions over a limited range of phenomena within its scope. It is only this feature which it shares with the reducing theory, and it is only this shared feature that is required by the notion of partial reduction.

4

More interesting then these cases of partial reduction are the cases of inhomogeneous reduction where the reduction is complete; that is, where the reduced theory is assimilated *in toto*, theoretical apparatus and all, to the reducing, and where the very fact of its being reducible serves to increase our confidence in its correctness. The reduction of the physical optical theory of light to the theory of electromagnetic radiation is of this nature and provides almost a paradigm of such a reduction.[1]

Nagel offers an interesting account of such inhomogeneous but total reductions.[2] He assumes, as I shall, that the concepts appearing in the reduced theory but absent from the reducing theory are not eliminable in terms of the concepts of the reducing theory by linguistic investigation alone. If such were the case, that is, of the obtrusive terms of the reduced theory were synonymous with expressions constructable out of the descriptive terms of the reducing theory, and if substitution for such terms of their equivalents allowed the derivational reduction of the reduced theory to the reducing, then the reduction would constitute a matter of linguistic insight and perhaps clever logical inference, but it would hardly require experimental justification and observational confirmation. But the reduction of, say, physical optics to electromagnetic theory is not of this sort, but is instead an important *factual* discovery of empirical science.

To account for such a reduction some connection must be found between the terms peculiar to the reduced theory and those found in the reducing, despite the fact that this connection cannot be supplied by assiduous use of dictionary, grammar book and logic text. Where can such a connection be found? Nagel rightly asserts that it must be an empirically justified scientific discovery. But what should its nature be? According to Nagel the connection between the two theories is to be found in the existence of a set of 'bridging' hypotheses or 'correlatory laws'. It is the discovery of these laws, containing concepts of the reducing theory *and* concepts peculiar to the reduced theory, which allows for the reduction of the one theory to the other. Originally Nagel insisted upon these correlatory hypotheses being universally quantified bi-conditionals, with one side of the bi-conditional containing as its only descriptive term one of the terms peculiar to the reduced theory. Subsequently he appears to have weakened this condition, allowing the laws to take other forms as well.[3]

But not just any set of correlatory laws will suffice to establish a reduction. For the purpose of the reduction is to show that given our reducing theory, *and* the correlatory laws, the independent assumption of the hypotheses of the reduced theory is no longer necessary. To meet this requirement we must insist that the correlatory laws meet the *condition of derivability*: that from the reducing theory and the correlatory laws the reduced theory can be derived by logic alone.[1] Let us call a body of 'bridging' hypotheses or 'correlatory laws' sufficient to allow us to deduce one theory from another with its aid a *correlatory theory* for the two theories, and the relation established by the existence of these laws a *correlation*. Its correlation an adequate analysis of total inhomogeneous reduction?

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[1] Like nearly all examples, this one fails to be quite adequate. There are predictions of traditional physical optics incompatible with some of those of electromagnetic theory, e.g. the latter's prediction of the exponentially decaying penetration of electro-magnetic waves into the surface of a reflecting opaque object. But the reduced theory can in this case, as in others, be suitably modified, without loss of any of its idiosyncratic concepts, so that the modified version of the theory is properly totally reducible to the electromagnetic theory.

[2] E. Nagel, 'The Meaning of Reduction in the Natural Sciences', reprinted in A. Danto and S. Morgenbesser, (eds.), *Philosophy of Science*, New York, (1960), pp. 288-312 and E. Nagel, *The Structure of Science*, New York, 1961, ch. 11.

[3] E. Nagel, *The Structure of Science*, pp. 355-6.

Unfortunately it is not. It is easy to show that correlation is insufficient for reduction, although necessary in a sense. First note its insufficiency. The Wiedemann-Franz law is a well-known law allowing one to infer the thermal conductive properties of a material from its electrical conductive properties. With it we can deduce from the laws of electrical conductivity of a material the laws of its thermal conductivity. But does this law establish the reduction of the theory of heat conduction to the theory of the conduction of electricity? No one has ever maintained that it does. What does explain *both* the electrical and thermal properties of matter, and the Wiedemann-Franz law as well, is the reduction of the macroscopic theory of matter to the theory of its atomic microscopic constitution. Although the correlation *points* to a reduction it does not *constitute* a reduction by itself.

But why doesn't this correlation count as a reduction? The answer seems clear. The derivation of the laws of the reduced theory, T2, from those of the reducing, T1, requires the introduction of the independent laws of the correlating set. Call their conjunction TC. But the fact that T2 can be deduced from T1 *and* TC is evidence only of the reducibility of T2 to the now expanded theory, 'T1 and TC'. This reduction is not inhomogeneous, since the conjunction of T1 and TC contains all the concepts present in T2; in fact, it is a simple derivational reduction. But this reduction is not the reduction of T2 to T1 alone originally sought for.

If we can find correlations which are not reductions, can we find reductions which display no set of correlatory laws? Not surprisingly we can. Although we find books on the theory of physical optics, and books on the theory of electromagnetic radiation, we fail to find texts on the theory (necessary to account for the reduction of physical optics to electromagnetic theory on Nagel's account) of the correlation of light with electromagnetic radiation. There are, then, reductions without correlations.

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[1] We might note that in its original form, with (1) the bridging postulates simple biconditionals containing each only a single one of the reduced theory's idiosyncratic terms, and (2) this only once on one side of the biconditional, and (3) with one bridging law for each of the terms peculiar to the reduced theory, the condition of derivability is unnecessary if we are interested only in reducing the reduced theory to *some* theory not containing the peculiar concepts in question. If we wish the reduction to hold to some *specified* theory not containing the peculiar concepts, or if our correlatory laws are not of the simple form described, however, the condition of derivability must be imposed as a separate requirement. See Nagel, ibid. p. 356 note.

But I have said that 'bridging' hypotheses are in some sense necessary to reductions, and I must now make that sense clear. What I shall propose is nothing which is surprising any longer, nor do I claim for it any originality. On the other hand it seems at least part of a correct account. It is that the place of correlatory laws is taken by empirically established *identifications* of two classes of entities.[1] Light waves are not correlated with electromagnetic waves, for they *are* electromagnetic waves. There are not two classes of entities, but only one; a class whose members may be referred to with equal correctness as light waves or as electromagnetic waves. The identification takes the form of a universally quantified conditional, or biconditional if the two classes are coextensive (which they are not in our example since there are electro-magnetic waves which are *not* light waves, although all light waves are electromagnetic waves); but it is a universalised conditional of a very special form.

The advantages of identification over 'mere' correlation have been dealt with at great length in recent literature, and I shall mention them quickly and without elaboration. I might note in passing that at least some of these advantages take on a particularly obvious aspect in the case of the reduction of the theory of the macroscopic properties of matter to the atomic theory, a reduction accomplished by the identification of macroscopic material substances with arrangements of atoms. In this case the very notion of a correlatory alternative to the identification seems peculiar. What would it be to *correlate* the piece of salt with an array of atoms at the same place and time rather than to identify the salt as an array of atoms?

Three distinct features of these identifications should be noted. First, these identifications are quite different from those identifications in which we identify some one individual referred to by one proper name with an individual referred to by a different name. Primarily this is because (1) the identifications are general, identifying whole *classes* of individuals, and (2) because the predicates used are not only meaningful, having sense as well as denotation, but have their sense due to their role in a scientific theory of wide scope, which theory provides connection by means of laws between these predicates and many others of the same, higher or lower (i.e. 'more directly observational') level.

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[1] Notice of the special role of identificatory statements was originally made by philosophers concerned with the mind-body problem. See, for example, U. Place, 'Is Consciousness a Brain Process', *Brit. Jour. Psych.* 47, (1956), 44-50; H. Feigl, 'The *Mental* and the *Physical*', in Feigl, Scriven and Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, 2, pp. 439 ff.; J. J. C. Smart, 'Sensations and Brain Processes', *Phil. Review*, 68, (1959), 141-56; H. Putnam, 'Minds and Machines', in S. Hook (ed.), *Dimensions of Mind*, New York, 1960, pp. 148-79.

Second, the identifications, despite the fact that they are universally generalised conditionals, differ radically from ordinary physical laws in that a request for an explanation of them is inappropriate. You cannot, as many have pointed out, ask *why* light waves are electromagnetic waves. What has not been as fully pointed out is that the identificatory process hardly 'eliminates' responsibility for explanations previously present. To a large extent, and this is a notion which needs far more looking into, the identification simply *transfers* the need for explanation to some other part of the theory. True, we cannot ask why light waves are electromagnetic waves. But we surely wish to know why entities like electromagnetic waves display the properties which we formerly used as our *criteria* for the presence of light waves. And the answers to these questions may be complex indeed. In our example, for instance, it will require explaining why electromagnetic waves affect our ability to *see* physical objects, and this in turn requires the identification of eyes and objects with complex arrays of electromagnetically interactive atoms and their components.

Third, despite the ability of identifications to 'simplify' our ontology, their ability to 'eliminate' unnecessary entities must be dealt with cautiously. It is true that after the identification there is only one *class* of entities, where before we thought there were two. But there are still as many light waves in the world subsequent to the identification as there were before, and as much commitment to them in our theory. This should be contrasted with, for example, the replacement of the caloric theory by the energetic theory of heat. According to the latter theory there simply is no such stuff as caloric. According to the electromagnetic theory of light there may indeed be light waves, and there are—lots of them. A new theory may bring about ontological simplification either by absorbing the ontology of an existing theory, or by eliminating it.

5

Although I have committed myself only to offering a taxonomy of reductions, and have deliberately eschewed questions of a general metaphysical and epistemological nature, I cannot resist the temptation to conclude by reviewing one or two considerations suggested by the nature of identificatory reductions. These may be of interest to philosophers more concerned with the metaphysics and epistemology of identification, than with its role in intertheoretic reduction in the natural sciences.

(1) How do we know when one class of entities is to be identified with a class of entities differently designated, and when the behaviour of the entities in one class is merely to be correlated with the behaviour of the entities in the other? The answer seems to be, although I imagine it is fraught with difficulties, that you identify whenever you can. That is, unless one has grounds for doubting the correctness of the identification, one should assume its correctness, even if a mere correlation would be equally compatible with all known facts.

(2) Must entities be spatial in order to be identified? Perhaps, but you need not have known they were spatial to begin with. Genes are DNA molecules, and since molecules are spatial objects, so are genes. Prior to the identification no one *knew* that genes were spatial objects, although many of course suspected they were. In any case the identification need not be established by first determining the spatio-temporal coincidence of the objects to be identified as some have maintained.

(3) Since identicals are indiscernable (we need not worry about intensional properties), then if *a* is identical with *b*, and if *a* has property *P*, must not *b* have property *P*? Of course. Even if it sounds odd to speak of *b* as being *P* it may be true that it is. Who would have thought that the statement 'The gene for blue eyes weighs less than a pound' made good sense? Would Mendel have found it to offend his sense of ordinary language? Perhaps so, but it's true anyway.[1]

One fact which should be noted with more care than has been given it, however, is that when we wish to identify *a* with *b*, and when we *think* that *a* has property *P*, and when we will not tolerate asserting that *b* has *P*, one way out is simply to deny that *a* really has property *P* after all. A standard procedure for doing this seems to be to 'push' property *P* into the realm of the mental where, if you are a physical scientist, it won't bother you any more. Thus, consider the following:

A: 'Apples are arrays of atoms.'
B: 'But how can apples be arrays of atoms? After all, apples are *sweet* aren't they? And arrays of atoms can't really be *sweet*?'
A: 'But apples aren't *really* sweet. The sweetness is *in the mind.*'

I do not say this is a *good* argument on A's part. Certainly as it stands it simply will not do. But is not something like this at least part of the impetus behind the theory of 'secondary qualities'? Anyone wishing to use the analysis of identificatory reduction in the natural sciences to illuminate a theory of the relation of mind to brain will have to deal with this problem with care. For in this case there is no mental 'receptacle' for inconvenient properties.

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[1] H. Putnam, ibid. pp. 166–9.

(4) The fact that it is *things* which one identifies in inhomogeneous reductions leads to a number of puzzles. In our identificatory 'bridging' hypothesis the variables must be taken to range over things, for example light waves which are electromagnetic waves, and our predicates must be substantival, for example, '. . . is a light wave' or '. . . is an electromagnetic wave'. If we reframed our physical theories in a 'co-ordinate language' in which the variables ranged only over space-time points, or over their co-ordinates, and in which the predicates were all property-attributing of the points, or their co-ordinates, then no distinction could be drawn between an identification of, say, light waves with electromagnetic waves, and the 'mere' correlation of the appropriate properties of points or co-ordinates. Whether this is an argument against the adequacy of co-ordinate languages for physical theories, or against the tenability of the distinction between correlations and identifications is an interesting question. I have no desire to perpetuate a 'useless metaphysical' distinction between substantival and property-attributing predicates, but if the differences between correlations and identifications noted above are real differences, then a distinction of this sort, metaphysical as it may be, seems to be far from useless. Unfortunately I am completely unprepared to offer even the beginnings of an analysis into just what this distinction, and the concomitant distinction of generalisations into general identifications and correlations, amounts to.

(5) A warning must be given against the two casual extension of the notion of identification to 'entities' of categories other than that of substance. It is all very well to identify light waves with electromagnetic waves, salt crystals with arrays of atoms or genes with DNA molecules; for all these entities are *things*. It is quite a different matter to 'identify' *properties* with one another, or *events* with one another, or *states of affairs* with one another, or *processes* with one another. It is clear that far too much mileage was got out of the identification of *things* in natural science by those attempting to treat the relation between mental processes and physical *processes* as an exactly analogous matter. Many of the criticisms of the 'identity' theory of mind make the same error. Those who claim that mental processes are not spatial, and hence not identical with physical processes, and those who claim that mental processes are shown to be spatial by their identity with physical processes, even though we did not realise their spatiality prior to the discovery of the identity, are equally careless. For no *process* is spatial in the sense that a physical *object* is

spatial, and this goes for *physical* processes as well as for mental processes!

It would be pleasant to dissolve the whole issue of the 'identity' theory of mind by showing that we never identify 'entities' except those in the category of things. But this is not so. We even 'identify' properties, as in 'The colour of the sky is blue', or 'The colour of rubies is red'.

(6) Lastly, there is one kind of 'reduction' upon which we have not even touched. This is the 'reduction' which proceeds by 'identifying' one class of referents with a class of 'abstract objects' associated with (instance in) but not identical to some particular physical objects. The 'reduction' of the *logical states* of a machine to its *physical states*, or of some 'mental' properties to 'patterns of functional organisation of the physical body' are of this kind. Such 'reductions', however they are to be analysed, are hardly the 'identificatory reductions' of the sort familiar from physical science.[1]

6

We have found that reductions are distinctive scientific accomplishments of very different kinds. Some are homogeneous, others inhomogeneous. Some are 'total', others merely 'partial'. Some are simply derivations, others elaborate identifications. Worse yet, some provide deeper confirmation for the reduced theory, while others serve to eliminate the reduced theory as a viable competitor for the status of scientific truth in the very act of reducing it to another accepted theory. For all their similarities, reductions are a very diverse bunch of items to come under a single name. But this should neither surprise nor disturb us so long as we do not let the convenience of the use of one 'family name' lead to obfuscation by equivocation.

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[1] For a discussion of this notion of reduction, cf. H. Putnam, 'The Mental Life of Some Machines', to appear in H. Castaneda, (ed.), *Wayne State Symposium in the Philosophy of Mind*, and J. Fodor, 'Explanations in Psychology', in M. Black, (ed.), *Philosophy in America*, London, 1965, pp. 161-79.

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The Concept of Homology in Biology*

by N. JARDINE

In this paper certain long-standing problems in the definition and interpretation of biological homology are discussed. A logical reconstruction of homology is proposed, and shown to generate a computable process for finding homologies. The relations between homology, embryology, evolution theory, and taxonomy are considered briefly.

1. INTRODUCTION

Most recent textbooks define biological homology in terms of common ancestry; e.g. De Beer ([6], p. 478), 'The sole condition which organs must fulfil to be homologous is to be descended from one and the same representative in a common ancestor', and Simpson ([28], p. 78), 'Homology is resemblance due to inheritance from a common ancestry'. Often it is contrasted with analogy, resemblance due to common function. A frequently cited example of homology is the relation between the arm of a man and the wing of a bat, and of analogy the relation between the wing of a bat and the wing of a butterfly.

Recently many writers have objected to this kind of definition of homology on the grounds that it is viciously circular, and the objection seems to be well-founded. By these definitions, in order to show that part x in one organism is homologous with part y in another organism, it must be shown that they are derived from a part z in a common ancestor. But homology itself is invoked in identifying parts x and y with part z. The objection is not merely that, since fossil series are incomplete, homology can only be inferred indirectly; however complete the fossil series, these definitions remain invalid, for homology is invoked even in identifying parts of parents with parts of their offspring. How has this muddle arisen?

2. AN OUTLINE HISTORY OF BIOLOGICAL HOMOLOGY

A central issue in the famous controversy between Cuvier and Geoffroy St Hilaire was the degree to which animals conform to a common type (see Piveteau, [24]). St Hilaire, 1818 [8], claimed that conformity to type was demonstrable by his *théorie des analogues*, whereby the analogy of

* Received 2.ix.66

parts was to be established by showing that they occupied corresponding relative positions in different animals. This criterion, the *principe des connexions*, he regarded as the only sure guide to analogy, on the grounds that—'an organ is rather changed, atrophied, or destroyed, than transposed' (translated), ([8], p. xxx). It is clear that St Hilaire's analogy would now be described as homology.

In 1843 Owen ([21], p. 374) defined ***homologue*** as—'The same organ in different animals under every variety of form and function.' In 1847 ([22], pp. 175-6) he distinguished ***special homology***—'the correspondency of a part or organ, determined by its relative position and connections, with a part or organ in a different animal', from ***general homology***—'a relation in which a part or series of parts stands to the fundamental or general type'. Owen's views on homology were derived from the idealist morphology of St Hilaire, but in distinguishing special homology from general homology he was, to some extent, discriminating between the empirical and idealist components of St Hilaire's analogy.

In 1859, in the section on morphology of chapter XIII of the Origin of Species ([5], pp. 434-5), Darwin wrote:

> Geoffroy St Hilaire has insisted strongly on the high importance of relative connexion in homologous organs: the parts may change to almost any extent in form and size, and yet they always remain connected together in the same order. . . . Yet in all this great amount of modification there will be no tendency to alter the framework of bones or the relative connexion of the several parts. If we suppose that the ancient progenitor, the archetype as it may be called, of all mammals, had its limbs constructed on the existing general pattern, for whatever purpose they served, we can at once perceive the plain signification of the homologous construction of the limbs throughout the whole class.

Darwin considered the occurrence of homology to be evidence for his theory, and his interpretation of homology was widely accepted. But his followers were not content merely to interpret homology in terms of evolution. In 1870 Lankester ([16], pp. 34-35) wrote:

> The term 'homology' belongs to the Platonic school, but is nevertheless used without hesitation by those who reject the views of that school. Professor Owen . . . would understand by ***homologue*** 'the same organ in different animals under every variety of form and function'. . . . But how can the sameness (if we may use the word) of an organ under every variety of form and function be established or investigated? This is, and always has been, the stumbling-block in the study of homologies without the light of evolutionism; for, to settle this question of sameness, an ideal 'type' of a group of organisms under study had to be evolved from the human mind, after study of the component members of the group; and then it could be asserted that organs might be said to be the 'same' in two animals which had a common representation in the ideal type.

In place of the objectionable term homology Lankester defined homogeny: 'Structures which are genetically related, in so far as they have a single representative in a common ancestor, may be called *homogenous*.' The term homogeny was not widely adopted, but its definition was widely accepted as a definition of homology.

Such definitions received apparent support from the Recapitulation Theory, in which it was supposed that stages in the development of individual organisms could be identified with stages in their evolutionary development. Hence, to show that parts were derived from the same part in a common ancestor, it sufficed to show that they had the same origin in embryology. For example, Gegenbaur ([7], p. 80) wrote in 1870: 'Special homology is the relationship between two organs which have had a common evolutionary origin, and which, as a corollary, have arisen from the same embryonic rudiment (Anlage)' (translated). In an important paper of 1895 Wilson ([32], p. 114) cited several cases in which obviously homologous adult structures differed widely in embryological origin, and concluded that:

> The very statement that homologous parts differ in embryological origin itself implies some higher standard of homology that outweighs that of development. What is that standard? Obviously it is the standard of Owen, viz., *the structure and structural relations of the developed organ;* it is the standard of comparative anatomy.

The Recapitulation Theory is now discredited, and with it the embryological criterion of homology.

After the development of classical genetics, various attempts were made to define homology in terms of common genetic determination, e.g. Haldane 1927 [10], and 1938 [11], and Boyden 1935 [2], but the definition of homology in terms of common ancestry has remained orthodox, e.g. Hubbs 1944 [12], and Haas and Simpson 1946 [9]. The persistence of this kind of definition may result, in part, from the failure of its opponents to agree on any simple alternative.

Many opponents have advocated, in one form or another, a return to pre-Darwinian idealist morphology. Thus Schindewolf 1937 [26], and 1950 [27], combines definition of homology in terms of common representation in an ideal type with a wholesale rejection of gradual evolution by natural selection. More moderate are the views of Kälin 1941 [14], and 1945 [15], and Naef 1926 [17], and 1927 [18], summarised by Zangerl 1948 [34], but even here there are idealist overtones. Thus Zangerl translates Kälin's ([15], p. 13) definition of homology: 'Homology exists, if parts of different organisms correspond to each other within the common structural plan and morphotype of a given systematic category', and endorses it on the grounds that it does justice to the holistic nature of the organism. Others have followed Wilson's suggestion that comparative anatomy should be the sole criterion of homology. For example, Boyden 1943 [3], and 1947 [4], considers *essential similarity* to be the sole criterion of homology, the components of essential similarity being ([4], p. 656): 'agreement in relative position and connections', and, 'agreement in adult structure or development or both'. Similarly, Remane 1956 ([25], p. 30) describes three principle criteria of homology: similar position in comparable systems, special qualities of structure, and connection by intermediate forms, and points out that homologies decided by different criteria may not coincide.

In an important paper of 1945 [33], Woodger proposed a reconstruction of homology using the criterion of agreement in relative position and connections. Informally expressed, Woodger defined homology as a 1 : 1 correspondence between parts of organisms in which a set of properties is preserved. The properties used are derived from the spatial relations between parts of organisms (anterior to, adjacent to, etc.). This reconstruction has been developed and clarified by Withers 1964 [32]. There are serious defects in the reconstruction. Both Woodger and Withers are forced to postulate a unique hierarchy of parts and sub-parts in every organism, and this is untenable since a part may be a sub-part of more than one other part. A process for finding homologies can only be derived from the reconstruction in special cases (for example only if all the relations are transitive).

The proposed reconstruction follows Woodger in using the criterion of agreement in relative position and connections, but homology is defined directly in terms of parts and relations, and a computable process for finding homologies is derived. Informally expressed, homology is defined as 1 : 1 correspondence between parts of complex entities in which a set of relations is preserved.

3. A LOGICAL RECONSTRUCTION OF HOMOLOGY

(i) *A logical model*

If X is a non-empty set, and R a set of non-reflexive relations in X, X will be said to be **coherent** in R.

If X and Y are two sets coherent in the same set of relations, R, and if there is a 1 : 1 function, f, from X onto Y, such that:

if $\mathbf{r}$ is a member of R,
and if x and y are members of X,
then $(x\,\mathbf{r}\,y) \equiv (f(x)\,\mathbf{r}\,f(y))$,

then the function, f, will be called a **similarity** with respect to R between X and Y.

Since a similarity with respect to R between X and Y is a 1 : 1 onto function, there is for each similarity a unique inverse similarity from Y onto X: i.e. similarity with respect to R is **symmetric.**

A similarity, f, between X and Y, and a similarity, g, between Y and Z, with respect to the same set of relations, R, together form a composite function, gf, between X and Z. Since:

if x and y are members of X, and $\mathbf{r}$ is a member
of R, $(x\,\mathbf{r}\,y) \equiv (f(x)\,\mathbf{r}\,f(y))$,
and hence $(x\,\mathbf{r}\,y) \equiv (gf(x)\,\mathbf{r}\,gf(y))$,

it follows that gf is itself a similarity: i.e. similarity with respect to R is **transitive.**

Let P and Q be sets of **disjoint** parts, P from a complex entity A, and Q from a complex entity B. Other pairs of sets of disjoint parts can be formed from P and Q, one set in each pair having as members:

either parts which are members of P,
or unions of parts which are members of P,
that are themselves parts,
or both; parts which are members of P, and
unions of parts which are members of P,
that are themselves parts,

the other set in each pair being formed similarly from Q.

If there is a unique largest similarity (i.e. a similarity having as members the greatest number of ordered pairs of parts), h, with respect to a set of relations, R, between a pair of such sets of parts, X and Y, then h is a **homology** with respect to R. The ordered pairs of parts that are members of h are pairs of **homologous** parts, or **homologues.**

Any smaller similarity, g, with respect to the same set of relations, R, between a pair of such sets of parts, Z and T, will also be called a homology if it obeys the following condition with respect to the unique largest similarity, h:

if x is a member of X, and if z is a member of Z,
then, $(x \subset z) \equiv (h(x) \subset g(z))$.

This will be called the C condition.

(ii) *An interpretation of the model*

If the complex entities, A and B, are interpreted as comparable stages in the life histories of two organisms; the set of relations, R, is interpreted as a set of spatial relations, e.g. anterior to, distal to; the sets of parts, P and Q, are interpreted as sets of parts, organs, or structures; ⊂ is interpreted as 'is contained in', or 'is a sub-part of ', so that the C condition may be translated as—if a part z is homologous with a part t, and z is a sub-part of x, then if x is homologous with any part, it is homologous with a part that has t as a sub-part (note that $t = g(z)$),

then homology may be interpreted as ***biological homology***, that is, the relation between parts which occupy corresponding relative positions in comparable stages of the life-histories of two organisms.

If the complex entities, A and B, are interpreted as disjoint parts of some stage in the life-history of a single organism, then homology may be interpreted as ***serial homology***, the relation between, for example, two vertebrae in a human body.

(iii) *A computable process for finding homologies*

Since similarity with respect to R has been shown to be transitive (see p. 129), the model can be extended to define an n-place homology with respect to R, having as members ordered n-tuples of homologous parts in n complex entities. The process given is derived from the extended model:

1. Choose sets of disjoint parts, P_1, P_2, . . . P_n, one from each of the n complex entities.
2. Choose a set of relations, R.
3. From P_1 form all sets of disjoint parts having as members:

either parts which are members of P_1,

or unions of parts which are members of P_1, that are themselves parts,

or both; parts which are members of P_1,
and unions of parts which are members of P_1, that are themselves parts.

Form sets similarly from each of P_2, . . . P_n.

4. Find any similarities which exist between n-tuples of sets of parts, one formed from each of P_1, . . . P_n, in 3. If there is a unique largest similarity, it is a homology with respect to R.
5. If there is a unique largest similarity, h, found in 4, check all smaller similarities found in 4 to see if they obey the C condition with respect to h. The similarities which obey the C condition with respect to h are homologies with respect to R.

If the complex entities studied have an axis, or axes, of symmetry, and the relations from which the set of relations, R, is to be chosen are spatial relations, constraints must be imposed upon **either** the sets of disjoint parts chosen in 1, **or** the set of relations chosen in 2. For example, in order to find a homology between parts of bilaterally symmetrical complex entities, **either** only sets of disjoint parts from one half of each of the complex entities must be chosen in 1, **or** one of the relations 'left of' or 'right of' must be chosen in 2. The constraints arise from the proof, given below, that a homology can exist only between sets of parts of a certain kind:

> **Definition.** For any set, X, coherent in R, there is at least one similarity with respect to R between X and X, namely the identity function, I. If I is the only similarity with respect to R between X and X, then X will be said to be **fully coherent** in R.
>
> **Theorem.** There is at most one similarity with respect to R between two sets fully coherent in R.
>
> **Proof.** If X and Y are two sets fully coherent in R, and if there are two similarities with respect to R between X and Y, g and f, then gf' (where f' is the unique inverse of f) is a similarity with respect to R between Y and Y. Since Y is fully coherent in R, gf' must be the identity function, I, and g the inverse of f'; but f is the unique inverse of f', so g and f cannot be distinct.
>
> **Theorem.** There is no unique similarity with respect to R between two sets that are not fully coherent in R.
>
> **Proof.** If X and Y are not fully coherent in R, there is a similarity, g, with respect to R between X and X, such that $g \neq \mathrm{I}$. Hence, if there is a similarity, f, with respect to R between X and Y, there is a distinct similarity, fg, with respect to R between X and Y.

From the two theorems it follows that all and only the similarities with respect to R between sets fully coherent in R are unique. Unless the constraints indicated above are imposed, none of the sets formed in stage 3 of the process will be fully coherent in R, and hence no homology with respect to R can be found.[1]

The following features of the process are important:

1. It is entirely a contingent matter whether, for given complex entities, and a given set of relations, R, different initial selections of parts will yield the same homology. Likewise, it is entirely a contingent matter whether, for given complex entities, and a given initial selection of parts, different sets of relations will yield the same homology. Remane's conclusion (see p. 128), that homologies decided by different criteria may not

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[1] A programme has been written by Miss A. A. Houston for Titan at the Cambridge University Mathematical Laboratory, to carry out stages 3 and 4 of the process. It has been used to compute the homologies between bones of a variety of vertebrate skulls. The results have corresponded to the homologies recognised intuitively in simple cases, and the method is being applied successfully to more complex cases (e.g. the skulls of teleost fish) where the recognition of homologies 'by eye' is more difficult.

coincide, remains true even within the basic criterion of correspondence in relative position. In computing biological homologies an obvious criterion in the initial selection of parts is that the parts selected should be as exhaustive as possible of the organisms, or parts of organisms, studied. But there is no such obvious criterion in the initial selection of relations in R, though in the studies so far undertaken the most consistent results are given by selecting the three relations 'anterior to', 'dorsal to', and 'left of' (or their converses), and only supplementing these with relations such as 'adjacent to' when no unique largest similarity is found.

2. The use of such relations as 'anterior to' requires identification of axes in the complex entities studied, and at first sight this introduces an element of circularity, for the identification of axes might be thought to depend upon a prior recognition of at least some of the homologies of parts. In fact no prior recognition of homology need be assumed. The correct identification of axes in given complex entities may be defined as that which yields the largest homology with respect to a given set of relations, R, for a given selection of parts. Thus it is possible to find the correct identification of axes experimentally by testing various orientations of given complex entities, to determine which relative orientation yields the largest homology.

(iv) *An illustrative example*

For the complex entities, A and B, shown in Fig. 1, the sets of parts, P and Q, selected in stage 1 of the process, have as members the volumes numbered 1-4 and 5-8 respectively. Unions of numbered volumes having surfaces in common are considered to be themselves parts. If the set of relations, R, selected in stage 2 of the process, has as members 'anterior to', 'ventral to', and 'right of', a unique largest similarity ($1 \cup 2$, 5) (3, 7) (4, 6) is found in stage 4 of the process. This unique largest similarity is a homology, or set of ordered pairs of homologous parts. Smaller similarities found in stage 5 of the process, which obey the containment condition with respect to the unique largest similarity and are hence homologies, are, for example, ($1 \cup 2 \cup 3$, $5 \cup 7$) (4, 6) and ($1 \cup 2$, 5) ($3 \cup 4$, $6 \cup 7$). This illustrates the case where a part in one complex entity is homologous with a union of parts in another, e.g. $1 \cup 2$ with 5. It illustrates also the case where a part in one complex entity lacks a homologue in another, e.g. 8 in B has no homologue in A.

If part 3 is omitted in the initial selection of parts, two largest similarities are found, ($1 \cup 2$, 5) (4, 6) and ($1 \cup 2$, 5) (4, $6 \cup 7$). Similarly, if the relation 'ventral to' is omitted in the initial selection of relations, two largest similarities are found, ($1 \cup 2$, 5) (3, 7) (4, 6) and (2, 7) (1, 6) ($3 \cup 4$, 8). This illustrates the fact that the process may yield different results for different initial selections of parts and relations in given complex entities.

It has been proved (see p. 131) that in symmetrical complex entities no homology of parts can be found by the process unless constraints are imposed upon the selection of relations in R. In the example the complex entities are bilaterally symmetrical, and hence no homology can be found unless one of the relations 'right of' or 'left of' is selected. If 'right of' is omitted in the initial selection of parts two largest similarities are found, $(1 \cup 2, 5)$ $(3, 7)$ $(4, 6)$ and $(1 \cup 2, 5)$ $(3, 6)$ $(4, 7)$.

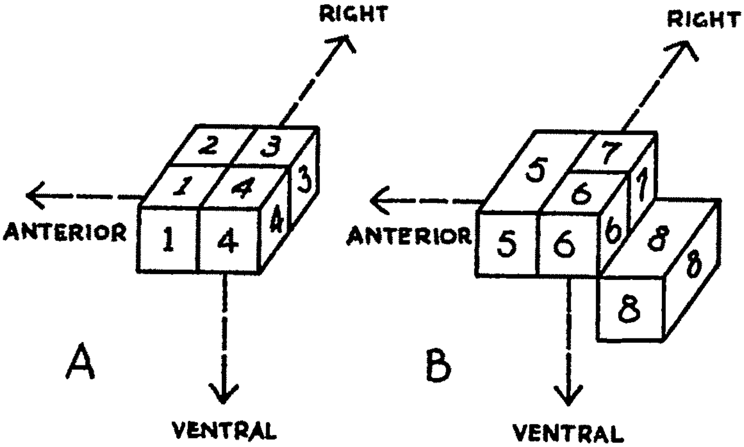

FIG. 1. Two complex entities.

4. HOMOLOGY AND EMBRYOLOGY

If many stages in the life-histories of two organisms are considered, a further condition on homology can be formulated. If h is a homology with respect to R between sets of parts X and Y from comparable stages in the life-histories of two organisms, if h' is a homology with respect to R between sets of parts X′ and Y′ from earlier comparable stages in the life-histories of the two organisms, and if D stands for 'develops from':

if x is a member of X, and if x' is a member of X′,
then, $(x \mathrm{D} x') \equiv (h(x) \mathrm{D} h'(x'))$.[1]

---

[1] This condition is analogous to the C condition (see p. 129), but the two conditions can be identified only if it is assumed that no migrations or reorientations of parts take place in the development of organisms; in other words, if it is assumed that, 'x is a sub-part of y at some stage in the life-history of an organism' if, and only if 'x develops from y at some stage in the life-history of the organism'. Woodger ([33], p. 100) makes this false assumption in identifying a postulated hierarchy of parts and sub-parts in adult organisms with a hierarchy based on the order of differentiation of parts in the development of organisms.

The condition may be regarded as a contingent statement about homology, and due to relative migrations and reorientations of parts in the development of different organisms it is in many cases false. As Wilson pointed out (see p. 127), there are many cases where obviously homologous adult structures differ in embryological origin. Alternatively, the condition may be regarded as an analytic statement about parts, on the grounds that it asserts, in effect, that parts are successively reidentifiable, successive reidentifiability being implicit in the concept of a part (cf. Strawson [30], pp. 31-38). If the condition is to be regarded as analytic the concept of homology must be restricted in one of two ways:

either the homology of embryonic parts (*h*′) may be used to restrict the homology of adult parts (*h*), so that the condition may be translated as—if a part *x*′ is homologous with a part *y*′, and if *x* develops from *x*′, then if *x* is homologous with any part, it is homologous with a part that develops from *y*′, **or** the homology of adult parts (*h*) may be used to restrict the homology of embryonic parts (*h*′), so that the condition may be translated as—if a part *x* is homologous with a part *y*, and if *x* develops from *x*′, then if *x*′ is homologous with any part, it is homologous with a part from which *y* develops.

The first of the ways of restricting the concept of homology is close to the discredited embryological criterion of homology. The second would sanction the use of the fate of parts in development ('prospective significance' is the term used by embryologists) as a criterion in determining the homology of embryonic parts. It seems best to have correspondence in relative position as the sole criterion of homology of parts in all stages of the life-histories of organisms, and hence, by regarding the condition as contingent, to deny that successive reidentifiability is implicit in the concept of a part of an organism.

5. HOMOLOGY AND EVOLUTION

Now that homology has been reconstructed, the circular definition of homology in terms of common ancestry can be formulated as a contingent statement about homology: if part *x* in organism A1 is homologous with part *y* in organism A2, then there is an organism B, ancestral to both A1 and A2, having a part *z* homologous with both *x* and *y*. If the *common structure* of a set of organisms is defined as the *n*-place homology between their parts, a more general hypothesis can be formulated: if a set of organisms (A1, A2, . . . A*n*), has common structure S, then there is an organism B, ancestral to each member of the set (A1, A2, . . . A*n*), such that the set of organisms (A1, A2, . . . A*n*, B) has common structure S′, and S′ has the same number of elements as S (the elements of S′ being ordered

($n+1$)-tuples of parts, and the elements of S being ordered n-tuples of parts).[1] This may be called the *evolutionary hypothesis*.

Many counterinstances to this hypothesis are known. For example, Osborn 1894 [19], and 1902 [20], describes many cases of apparently homologous, but independently evolved, cusps and folds in mammalian teeth. Such cases he called latent, or potential, homologies, and Hubbs 1944 [12] called independent homologies. Other writers, e.g. Haas and Simpson 1946 [9], include such cases under homoplasy (a term first used by Lankester [16]), defined by Simpson 1961 ([28], p. 78) as, 'resemblance not due to inheritance from a common ancestry'. In defining homology as, 'resemblance due to inheritance from a common ancestry', Simpson includes under homology not only cases of correspondence between parts in relative position, but also cases of resemblance between parts with respect to some set of attributes. The kind of confusion which can result from this is illustrated in the following quotation from Huxley, 1942 ([13], p. 395): 'For homology . . . no longer implies descent from a common ancestor showing the common feature. Two white-eyed mutant strains in two species of *Drosophila* are not descended from any common white-eyed ancestral strain.' By Simpson's criterion it seems that parts of two organisms (in this case eyes) may be at once homologous in one sense (correspondence in relative position), and non-homologous in another sense (resemblance with respect to some attribute or set of attributes).

Here it is suggested that a basic distinction be made between two kinds of relation between parts of organisms:

1. Correspondence in relative position; that is, ***homology with respect to some set of spatial relations***. Homologies that satisfy the evolutionary hypothesis may be called ***phylogenetic homologies***.
2. Similarity in form, structure, or appearance; that is, ***resemblance with respect to some set of attributes***. Although this distinction cuts across Simpson's, and most other recent authors', *definition* of homology, it is in line with the widely prevalent *use* of the term homology as a descriptive term in comparative anatomy.

A further confusion in comparative anatomy surrounds the use of such terms as common structure, structural plan, general plan, type, and archetype. The common structure of a set of organisms (defined on p. 134), is, informally expressed, the set of parts common to all members of the set of organisms. The concept of common structure is quite distinct from the concept of general plan, type, or archetype. The common structure of the mammals, for instance, lacks teeth since there are some toothless mammals, whereas the general plan, or archetype of the mammals is conceived as having a full complement of teeth. The process given for finding homologies can show where a part in one organism lacks a homologue in another. But if the general plan or archetype of a set of organisms is to be derived from its common structure, there must be some criterion for deciding if this indicates 'loss' of a part in one organism, or 'addition' of a part in the other. In other words, if the general plan or archetype is to be derived from the common structure of a set of organisms, there must be some criterion for deciding whether presence or absence of a part is the primitive condition in a set of organisms, so that 'missing' parts can be reinstated. Common structure, like homology, can be regarded as a descriptive term in comparative anatomy, but the general plan or archetype of a set of organisms is best regarded as an hypothesis about the structure of a common ancestor.

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[1] Woodger ([33], p. 109) gives a somewhat similar 'evolutionary postulate': 'If two lives exhibit the same Bauplan, then there is an ancestor common to both which exhibits a Bauplan which is exhibited by all three lives.' His definition of Bauplan is close to the definition of common structure given here, but the postulate is trivial since it does not preclude the case where the latter Bauplan has fewer elements than the former, i.e. where some parts homologous in the two lives lack homologues in the common ancestor.

The confusion between common structure and general plan, or archetype, is illustrated in the following passage from Woodger ([33], p. 113). His definition of Bauplan is close to the definition of common structure given here, but he writes:

> What has happened is that as a result of years of patient labour by generations of morphologists the principal Bauplans and their overlapping and other relations have been discovered, and it is these . . . which determine (in the logical sense of the word) the inclusion relations of the major taxonomic groups.

Now the common structure of high level taxa such as the *Mammalia* boils down to a few diagnostic features, and many low level taxa, e.g. *Homo sapiens* and *Homo neanderthalensis*, differ not at all in common structure, but only in the attributes of parts of the organisms belonging to them. Woodger is here using the term Bauplan as synonymous with general plan or archetype. He is, in effect, advocating a return to the pre-Darwinian theory that taxonomic systems are constructed on the basis of direct intuition of the archetypes of sets of organisms. Although homology yields no such short cut to the construction of a taxonomic system, it is, as will be shown in the next section, of considerable importance in the process of classification.

6. HOMOLOGY AND TAXONOMY

'Dr Sneath said that it was with embarrassment that the biologist had to admit that his primary information was often of a very poor sort. A lot of it turned on the question of what one should consider the same in two organisms' ([1], p. 255). This is an important point. If a classificatory system is to be constructed on the basis of estimates of resemblance between sets of organisms, there must be rules for the selection of criteria, usually called characters by taxonomists, upon which to base such estimates of resemblance. It is generally agreed that one such rule is that only characters that are in some sense 'the same' should be selected.

Sneath and Sokal ([29], pp. 69-74) attempt to clarify this notion of sameness of characters in terms of what they call operational homology. In their account of operational homology they suggest that sets of homologous parts be regarded as 'organ taxa', defined by correlation in some set of properties, and hence that operational homology be regarded as resemblance between parts with respect to some set of properties. This seems to be derived from a misunderstanding of the use of the term property in Woodger's paper [33], of which they give a brief account. They claim that, 'such reasoning leads us to the threshold of an empirical "numerical homology" '.

Here it is suggested that a rule for the selection of characters for use in a taxonomic study is as follows: a character must be such that its states are *either* attributes of whole organisms in all the organisms studied, *or* attributes of parts homologous in all the organisms studied. Thus, the character 'petal colour', having as states, 'petals red', 'petals blue', etc., should only be used where the parts called petals are homologous in all the plants studied. Many quantitative methods for the estimation of resemblance between sets of organisms, and for the construction of taxonomic hierarchies on the basis of such estimates of resemblance, have recently been published. The value of such methods must remain in doubt until precise rules are devised for the selection of the characters upon which the estimates of resemblance are to be based. One such rule is given above, and the process given in 3(iii) constitutes a numerical homology, though not of the kind foreseen by Sneath and Sokal.

The process of finding homologies, that is, identifying parts of complex entities which correspond with respect to some set of relations, may be called *primary classification*. Taxonomy, the process of grouping or arranging sets of complex entities on the basis of their resemblances, may be called *secondary classification*. Any reconstruction of the process of secondary classification will depend upon a reconstruction of the process of primary classification. It is the latter that has been attempted in this paper.

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Discussions

REMARKS ON SWANSON'S THEORY OF MODELS

Professor Swanson, if I understand him aright, makes the point that scientific models are used as metaphors, and that the metaphorical function of a model is such that the empirical determination of its usefulness is possible by means of a comparative analysis of its Rational Preference Ranking (or RPR for short) with that of the theory, viewed in the light of that metaphor.[1] I should like to examine this theory in the context of the only concrete example mentioned, that of the Bohr-Rutherford theory of the hydrogen atom.

I shall begin by considering the proposition that the structure of the model is imputed to the theory in formation. More specifically, I shall contend that, if 'model' is understood in the way physicists ordinarily use that term, then it was not the structure of the model that was carried over to the theory, but rather the model that was designed specifically to represent this particular aspect of the theory. And that, if by 'model' we understand 'analogue', then the similarity of structure between the analogue and the theory was due to the similarity of form between the gravitational and the electric field equations, and not to the imputation of the structure of the analogue to the theory. I shall then conclude that Swanson's theory of the metaphorical use of model, with its interesting feature of an RPR, not being applicable to the case of the Bohr-Rutherford theory of the atom, is still in want of philosophical justification.

Assuming, therefore, that by 'model' is meant what physicists ordinarily mean by that term,[2] I shall begin by recalling briefly how the Bohr-Rutherford model of the atom came to be advanced. Since the discovery of the electron in 1897,[3] it was known that the neutral atom contained both positive and negative charges. One of the questions that arose immediately was that of their distribution inside the atom. One possibility, that was advanced by J. J. Thomson, was that the electrons were imbedded more or less uniformly in a positive sphere of electricity having the dimensions of the atom, and in such a way that they could vibrate about their positions of equilibrium, and thus account for the characteristic lines that spectroscopists had discovered. A series of experiments had been carried out by Geiger and Marsden,[4] in which they bombarded thin foils of Ag or Au with alpha particles from a radioactive source, in order to observe the scattering patterns. They observed both backward and wide-angle forward scattering, which were inconsistent with the multiple scattering model of Thomson. It was to account for this scattering pattern that Rutherford, in 1913, advanced his theory of the nuclear atom, characterised by a large positive nucleus surrounded, in some fashion, by the negative electrons.[1] Unfortunately, given the Coulombian attraction between oppositely charged particles, such a disposition of charges cannot possibly be stable, as shown by Earnshaw's theorem. Thus, in order to preserve the observed stability of the atom, the electrons had to be made to orbit the nucleus. But then it could be argued—as indeed it was—that the electrons could not possibly be thought of as planet-like objects, since otherwise they would radiate energy, being electrically charged, and would, according to the electro-magnetic theory, spiral down to the nucleus with the result that, taking into account both electromagnetic and mechanical theories, the universe would have lasted about 10^{-14} sec. More important still, the radiation emitted would shift to higher and higher frequencies as the spiralling electrons came closer and closer to the nucleus, which is contradicted by the fixed frequencies of the emissions lines characteristic of the atom.

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[1] J. W. Swanson, 'On Models', this *Journal*: 17, No. 4 (1967), 297.

[2] Cf. e.g. P. Achinstein: 'Models, Analogies and Theories', *Philosophy of Science*, 31 no. 4 (1964), 328.

[3] J. J. Thomson, *Phil. Mag.* 44, (1897), 293.

[4] *Proc. Roy. Proc. Soc.* 82 (1909), 495; *Roy. Soc.* (London), 83 (1910), 492.

It was at this juncture that N. Bohr advanced a theory of the structure of the atom and of the origin of spectra, that brought together Rutherford's notion of the nuclear atom and Plank's theory of the quantisation of radiant energy, and so presented a new version of the nuclear atom.[2] For Bohr's model was certainly not planetary in the sense that Swanson takes it to be. For one thing, each electron had a set of several orbits assigned to it, and not just one. Secondly, the electron was allowed to jump from orbit to orbit without, however, being anywhere (mechanically speaking) during its orbital transitions. Thirdly, it did not radiate while in any one of its orbits, contrary to the classical laws, but restricted its radiating to those inter-orbital transitions that brought it to a state of lower energy from a higher, or 'excited' one. Fourthly, the shape, and more importantly the spacings between permitted orbits (or states, as Bohr called them), were determined by quantum conditions (that the angular momentum of the electron —the so-called angular momentum quantum number l—should be an integral multiple of $\hbar$).

From this it is evident, I think, that the size and shape of orbits, their number and spacing, the behaviour of the electrons on them and in transition between them, were determined entirely by the demands of the theory in formation, as was indeed the very conception of the nuclear atom. The Bohr model is hybrid, as I said, being the product of an extremely odd marriage between widely different theories, through the use of strangely incompatible notions, and no physicist would have developed it, unless compelled to by the weightiest of reasons. In other words, the model did not pre-exist the theory, nor was the theory (that of the hydrogen atom) structured after it. Rather, the model was designed to 'represent' the theory. The theory itself, of which this model was the representation, was designed to bring together, in some sort of organic unity, all that was known about the atom at the time. So that, if we should wish to talk in terms of imputation of structure, we should talk about the structure of the theory being imputed to the model, since the latter clearly was made in the image of the former—indeed, it is an image of it—and not the other way around, as Professor Swanson would have it.

That it is the structure of the theory that governs that of the model is further shown by this, that when the model can no longer be altered to represent that structure, because it becomes unmanageably complicated, or when the characteristics it exemplifies cease to be regarded as significant, it is simply abandoned. In all cases, however, it is clear that it is the structure of the theory that is imputed to the model and governs it. Indeed, one may even question the appropriateness of the expression 'imputation of structure' to indicate what is traditionally called 'representation'.[1] For to say that the structure of the model is imputed to the theory is to say, among other things, that the model pre-exists the theory which, as I have indicated, is not the case (i.e. the model has no 'private life' independently of the theory). So it is clear that, if by 'model' is meant something like the Bohr-Rutherford model of the atom (which is, by and large, the common meaning of that term among physicists), Swanson's theory of model makes no sense at all. The only other possible alternative, it seems, is that by 'model' he really means 'analogue', thus having confused the two in a way that is common among model theorists.[2] Let us try this approach. The analogue that is meant, in the case of the Bohr-Rutherford theory of the atom, is evidently the planetary system of classical mechanics. What is the root of the analogy, and what was its role in the development of the theory of the atom? The root of the analogy is clearly the central part occupied in the atomic model by the positive nucleus, due to its very large mass relative to that of the electrons. The electrons are thus imbedded in a central force field, with a Coulombian force of attraction inversely proportional to the square of the distance. Therefore the system, which is stable, must be in dynamical equilibrium, hence, one may reason that, *by analogy with* the solar planets, the negative charges must run about the nucleus on conical orbits—in this instance, as it turned out, on circular ones. This account, however, is defective in an important respect. The conclusion that electrons orbit the nucleus on circular paths was said to be drawn *by analogy with* the mechanical planetary model. But this is not so. For once the separation between nucleus and electrons had been made, on the basis of the experiments of Geiger and Marsden, and given the Coulombian law of attraction, there was no alternative open from a mechanical point of view other than the planetary model, and thus comparison with the solar system was inescapable, the latter being thoroughly familiar to all by that time. So that, up to that point, nothing of significance can be said to have been transferred from the analogue to the theory, the analogous results regarding orbits being guaranteed, as I have said, by the similar form of the gravitational and electric fields, and by the facts that the heavy nucleus was separated from the lighter electrons, and the whole atom electrically neutral and dynamically stable. So, the analogy between the atomic and the planetary systems, having been of no use so far, may be expected to have some significant import from this point on, if the 'un-mapped substructure of the model' is carried over to the theory in formation. But what happened in the case under examination? It was found, as I indicated earlier, that the electrons *could not* be thought of as planet-like objects, mainly because they were electrically charged, and also because the theory had to account, not just for the dynamical stability of the atom, but also for the origin of the spectral lines. And so we are faced with this most puzzling situation: the mechanical properties of the atom, to the extent that they correspond to their planetary analogue, were obtained independently of it, thanks to the form of the Coulombian field equation; and its other characteristic properties, such as its emission lines, were dramatically at variance with any possessed by the analogue, to such an extent that they were not even accountable for on the basis of *any* classical theory, electromagnetic or otherwise. Hence, the temporal priority of the planetary analogue, on which so much depends in Swanson's scheme, is clearly irrelevent, due to the fact that the analogue itself is otiose, being of no use for the determination of the mechanical properties of the atom, and nugatory, not to say nocent, for the determination of its spectral properties.

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[1] *Phil. Mag.* **21** (1911), 669.

[2] *Phil. Mag.* **26** (1913), 1.

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[1] H. B. Curry, 'Some aspects of the problem of mathematical rigor', *Bull. Am. Math. Soc.* 47 (1941), 221-41.

[2] Cf. e.g. P. Achinstein, op. cit. and 'Theoretical models', this *Journal*, 16, xvi, 62 (1965), 102.

Hence, we must conclude that Swanson's theory is not very useful in the context of the Bohr-Rutherford theory, which he mentions as an example. For, if to view the model as a 'metaphorical lens' or filter, is to say no more than that the model represents some significant feature of the theory, it is clear that nothing strikingly new has been said, since this is precisely what a model is constructed for. If, on the other hand, it is meant to say that an analogue, antecedent to the theory, is 'carried over' to the theory for purposes of comparison, in order to determine a preferential interpretation of that theory, then one may ask how characteristic of analogues is this view. For it is clear that—save as a pedagogical device of a crude sort—the planetary model borrowed from mechanics is not a metaphorical filter for viewing the theory of the hydrogen atom, since, as I have tried to point out, some significant features of the analogue are conspicuously absent from the Bohr-Rutherford theory, and vice versa. So that if the Bohr-Rutherford theory is really a case in point, then the only support one may find in physics for the metaphorical view of models is in the pedagogical use of crude similes. These, however, have no significant use in the process of theory formation, which is the one at issue here.

There is one last point that I should like to mention briefly before concluding these remarks, namely that of the feasibility of a comparative study of the RPRs characteristic of the analogue and the theory respectively, which is essential as a means of determining the degree of 'carry over', and therefore, presumably, of the usefulness of the analogue.

The method that is proposed for the determination of the RPR is empirical, and this, indeed, is its main virtue. The question, however, cannot long be escaped: 'how would one go about determining the RPR of the theory in formation?'. For it seems that, to the extent that the theory is truly in formation, there is no 'concensus RPR' to which one might appeal to establish the degree of 'transfer' from analogue to theory. Whenever the theory users all agree on a certain priority among the propositions of the theory, the theory is, to that extent, formed. To say that a theory is in formation is to say, at the very least, that there is no agreement among its users, on the ways its propositions are linked together, and ordered so as to form an organic whole. We may even go so far as to suggest that the formation of a model is an attempt to discover missing links between such groups of propositions, and that once the model becomes coherent, the theory that it represents becomes so as well. This is not meant to deny that a theory, at some stage in its development, may be formed by analogy with another theory, or part of it. What is at issue here, is the possibility of comparing the RPR of the analogue with that of the theory in formation, since I fail to understand what the RPR of such a theory would be.

These basically negative comments should not be construed to mean that Swanson's notion of an RPR is necessarily ill taken. I think it a valuable tool in our attempt to understand the way in which metaphors work, and the degree to which they are successful. But its application to scientific discourse is vitiated, I think, by Swanson's failure to dwell at greater length on the process of theory formation, that is, by his attempt to apply to the language of science in general, and of physics in particular, a pre-conceived theoretical notion without first examining how physics may differ from ordinary language in this respect.

To conclude. My point, essentially, in this: that Swanson has yet to show, by means of an appropriate example, how this theory of the metaphorical use of analogues applies to scientific discourse, and how the comparative study of RPRs can be performed on a theory that is in formation.

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HUMAN AND MACHINE LOGIC

The following summarising paragraph contains a few terms not defined until later:

Given any consistent formal system containing arithmetic, a man who has understood Gödel's construction can write down a true theorem G expressible in the system but unprovable in it. (The man will believe G if he believes the system is consistent.) A machine program that represents the formal system will never print G, even if the program contains a randomising device enabling it to apply the rules of inference in an arbitrary order. It has been argued that this shows that a man, *qua* mathematician, transcends a machine in at least one respect. (The argument does not depend on whether machines are capable of belief, nor on whether they could act as if they were so capable. I think they could so act[1] but the reader need not worry about this point since the present note is concerned with logic, not with probability.) This point of view is essentially refuted by the observation that Gödel's construction could itself be carried out by another (deterministic) machine. But further Gödel propositions can then be appended and a complete treatment leads inevitably to questions concerning the formalisation of transfinite counting, which incidentally preceded Gödel's construction historically by several decades. If the mentalists still wish to make a case they must base it on transfinite counting rather than on Gödel's theorem. It is entertaining to note that transfinite counting can be vividly expressed in polytheological terms. So much by way of introduction.

Lucas[2] argued that 'Gödel's theorem seems to me to prove that Mechanism is false, that is, that minds cannot be explained as machines'. Feeling that mathematical results can sometimes be proved by metaphysical arguments, but not conversely, I argued[1] that there must be some loophole in Lucas's thesis. But my discussion itself contained an error so I am anxious to argue the case again with greater accuracy. This accuracy is bought at the cost of an increase in technicality, but I believe the arguments will be intelligible at least to all philosophers of science.

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[1] I. J. Good, *Computers and Automation*, 8 (1959), 14-16 and 24-26.

[2] J. R. Lucas, *Philosophy*, 36 (1961), 112.

Given any computer program (formal system) for proving theorems in arithmetic, Gödel's construction[2] enables us to print new theorems which the original program would not print, and the new theorems are true if ordinary arithmetic is consistent. Let us express that more carefully.

A (*finitely based*) *formal system* is defined in the following manner. We are given a finite alphabet of symbols, and a finite set A of finite strings of these symbols, each string being called an *axiom*. Axioms are regarded as a special case of *theorems*. We are also given a finite set R of *rules of inference* which can operate on some finite sequences of theorems, each such operation produces a new theorem (again a finite string of symbols) and this theorem is then said to be *proved*. We call this the formal system $F = (R,A)$.

Corresponding to any such system, a computer program can be written which will print in turn each theorem of the system. The number of theorems is usually infinite, but *each* theorem (if provable) will ultimately be proved (printed).

The Gödel construction C can be applied to any finitely based formal system F, provided that ordinary arithmetic is represented in the system, and will yield a new 'theorem' or 'proposition' (string of symbols) G, called a Gödel proposition. This proposition will denote an arithmetical statement that is true in the formal system F, but is unprovable provided that F is *consistent*. (In an *inconsistent* system every proposition is provable including the 'false' ones such as $0 = 1$.) Moreover, if a system F_1 is consistent, its Gödel proposition G_1 can be appended to its set A_1 of axioms, and the new system F_2 will still be consistent.

In order to avoid a proliferation of notation, let us denote by F a program that prints the theorems that the system F can prove. Now the Gödel construction C can itself be expressed as a program, which we also denote by C. When C operates on the program F_1, it produces a program F_2, this being a representation of the formal system F_2. Since the program F_1 *is itself a string of symbols* we begin to regard the program C as itself a formal system, but since it can be applied to every program of the form (R,A) it might be very difficult to show that it is finitely based.

The new formal system F_2 satisfies the requirements for the application of Gödel's construction. This will give rise to a new Gödel proposition G_2 and a new formal system F_3, and so on.

We can imagine a human operator playing a game of one-upmanship against a programmed computer. If the program is F_n, the human operator can print the theorem G_n, which the programmed computer, or, if you prefer, the program, would never print, if it is consistent. This is true for each whole number n, but the victory is a hollow one since a second computer, loaded with the program C, could put the human operator out of a job. And even the original computer, suitably programmed, would be able to print in turn each of the Gödel propositions G_1, G_2, G_3, . . . , by repeatedly applying the operator C. By means of a process known to logicians as *triangularisation*, this program could be modified to print each of the theorems of the infinite sequence of formal systems F_1, F_2, F_3. . . . It is natural to denote this program by F_ω, where ω is the first *transfinite ordinal*. In spite of appearances, F_ω could be written in finite terms and corresponds to a finitely based formal system (R_ω,A) where A is the same finite set of axioms that we started with, that is, the axioms of the system F, and R_ω is R plus a finite number of extra rules.

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[1] I. J. Good, *New Scientist*, **26** (1965), 182-3, and letters in *New Scientist*, 27 May and 26 August, 1965.

[2] K. Gödel, *Monatshefte für Mathematik und Physik*, **38** (1931), 173. English translation by B. Meltzer published by Oliver and Boyd, Edinburgh and London, 1962.

The notion of transfinite ordinals can be thought of in terms of polytheism. We imagine that, for each integer n, ZEUS_{n+1} made ZEUS_n, where ZEUS_ω made ZEUS_n for all n. Who made ZEUS_ω? Answer: $\text{ZEUS}_{\omega+1}$, and the suffixes can be continued indefinitely, thus: $1, 2, 3, \ldots, \omega, \omega+1, \omega+2, \ldots, 2\omega, 2\omega+1, \ldots 3\omega, \ldots, \omega^2, \ldots, \omega^3, \ldots, \omega\omega, \ldots, \omega\omega^{\omega}, \ldots$. Similarly, the Gödel construction C can be applied to F_ω, giving $F_{\omega+1}$, and we can proceed to higher and higher systems, just as in the process of transfinite counting, sometimes adding 1, and sometimes applying a generalisation of triangularisation.

In order to write a program that can carry out this construction as far as any specifiable ordinal, it will be necessary at least to invent a representation for this, and for all smaller ordinals, on the integers. There is no known complete process for doing this, and the process of transfinite counting is thus naturally described as 'creative'. The use of this term is not evidence that the process cannot be formalised, and I believe that a sufficiently well-written program would be able to go as far in transfinite counting as any man can ever go. It is useless for the 'mentalist' to argue that any given program can always be improved, since the process for improving programs can presumably be programmed also; certainly this can be done if the mentalist describes how the improvement is to be made. If he does not give such a description, then he has not made a case.

A similar controversy applies in a wider context: the only reason I know to suppose that the creative intellectual processes of man cannot be mechanised is the weak one that it has not yet been done. I am of course here ignoring such practicalities as cost.

If the controversial 'axiom of choice' is true, then there is a smallest unconstructible transfinite ordinal τ. The question of its 'existence' is somewhat controversial, but of course it cannot be reached by any transfinite counting program. The controversy is bound up with what is meant by mathematical existence. ZEUS_τ should have a prominent place in any polytheology.

Some readers will have asked themselves what meaning it can have to say that a proposition, expressible in a formal system, is 'true' if it is not provable. One answer is that a proposition P, of finite length, can express an infinite number of provable propositions, and yet perhaps not itself be provable, since a proof, by definition, must be finite.[1] An example of a proposition that might be of this form is 'For all positive integers, r, s, t, and n, we have $r^{n+2}+s^{n+2} \neq t^{n+2}$'. This is of course the famous unproved 'Fermat's Last Theorem'. Like Riemann's hypothesis concerning the zeros of the zeta function, this 'theorem' might be true but unprovable but if false it is provably so.

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[1] I assume here an 'infinite axiom' of truth: that the conjunction of any number of true propositions is true.

An error in my *New Scientist* article, which was pointed out by Alan L. Tritter (who has also made many other useful suggestions), was in the assumption that the finiteness of the internal storage of a computer (or man) would prevent it (or him) from attaining some constructible infinite ordinals. The limitation cannot be in the finiteness of the internal storage, since it has been proved[1] that a universal computer (Turing machine) needs no more than one binary digit of internal storage (when its input-output tape is of unbounded extent, and the tape alphabet large enough). Of course such a computer would be intolerably slow, but that is beside the point.

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[1] E. C. Shannon, in *Automata Studies* (Princeton University Press, 1956), p. 157.

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Reviews

L. Pearce Williams: *Michael Faraday: A Biography* (London, 1965, 70*s*.) Pp. xvi+531.

Given the underdeveloped state of historical research on nineteenth-century physical science, L. P. Williams's *Michael Faraday* is a notable achievement. Working virtually without relevant models, Professor Williams has assembled a massive and factually authoritative survey of Faraday's career from a scrupulous examination of published and unpublished sources, some previously unknown, most previously unexploited by scholars. Any future study of Faraday must start from this book, and no student of British science in the last century may ignore it. Nevertheless, whether as an analysis of Faraday's career or as a model for future research, the book has numerous and essential imperfections. Some of these may affect the future of nineteenth-century studies. In this place and at this comparatively late date, I shall therefore largely take for granted the work's great virtues as an authoritative source both of data and of problems for research and concentrate instead on those things which Williams has failed to do or has done badly.

As a portrait either of an individual or of a scientific career in nineteenth-century Britain, the book is disappointingly routine. Williams's predominant concern is with Faraday's scientific ideas and their philosophical background. Fewer than one-third of his pages are reserved for an account of the main extra-scientific events in Faraday's life together with brief essays on such topics as 'The Sandemanian Church', 'The Royal Institution', and 'Faraday, the Educator'. Those pages add little except reliability to past accounts. Given the nature of Faraday's achievements and of Williams's interests, the distribution of space may well be appropriate. But it is unfortunately not just a distribution but a division. The two parts of Williams's narrative seldom interpenetrate or illuminate each other. What results is close to being two separate books.

Of course Faraday was a giant only in his science, but he came from what was, in Britain, representative stock. As a self-educated scientist and as a member of a dissenting sect (perhaps also as a married but childless physicist), Faraday belonged to groups which were disproportionately represented in British science during the first half of the nineteenth century. The Royal Institution, with which his entire scientific career was associated, was also a uniquely British organisation. Its foundation, its form, and its ultimate success are all symptomatic of forces which, during Faraday's lifetime, produced an almost total reorganisation of British scientific institutions and education. As incidental facts about Faraday, Williams supplies all these bits of information, but readers who see them as parts of a British pattern, unimaginable in France or Germany, must supply that viewpoint for themselves. Those able to do so will, I think, be disconcerted to discover how little this full-scale biographical study adds to their understanding of scientific life in Faraday's nation and time.

Nor is Williams's portrayal of Faraday, the man, more successful. This biography supplies many of the requisite fragments of a personality but not a human being plausible to the twentieth-century reader. Probably Williams is right to refuse (p. 132, n. 15) to speculate about the *causes* of Faraday's 'giddiness, loss of memory, and headaches throughout most of his adult life', but he might have supplied some sense of what life and work were like for the scientist who suffered from these disabilities. For him they are part of the chronology but not of the man. Thus he writes on page 102: 'The headaches were to increase in severity . . . as the years went by. The forced vacation in 1828 was to be repeated many times. The pattern of his life was formed by the alternation of work and rest'. Four pages later, however, Williams continues: 'Secure in his faith and beloved by his wife and relatives he was able to devote his entire energy for forty years to the task of uncovering one corner of [the truth].' Can this representative bit of rhetoric be about the man whose disabilities Williams has just chronicled?

Similar problems of reconciliation arise elsewhere. For example, the Faraday who 'was guided only by the love of truth' (p. 106) never acquires the complexity to be also the man who was repeatedly concerned to establish his priority in scientific discovery. As a result, one repeatedly wonders whether to take Williams seriously on those occasions when he does supply his subject's innermost thoughts: 'But, inside, [Faraday] must have wondered if he were not in error; if the simplicity and consonance of his theory with observed phenomena were not merely an illusion buttressed by his own ignorance' (p. 18). And, beyond this, there are many points of information for which the reader will search in vain. Which of the mature Faraday's colleagues and correspondents were friends, which acquaintances? What, in a personal or a scientific way, did he draw from them? Williams excerpts many letters, some of which would surely illuminate points of this sort. But the reader must still guess at answers if he is to make Faraday comprehensible as a man. Because they usually quoted at greater length, the nineteenth-century British 'lives', on which the biographical components of Williams's narrative seem modelled, often inadvertently provided more clues.

Probably it was a mistake to attempt the full biographical mode. Williams's main objective, which occupies most of the book, is an account of the development of Faraday's experiments and ideas, based principally on the published *Diaries* and *Experimental Researches*. Here he has been vastly more successful and might have been still more so in the absence of the concern with biography. With his attention focused on his central character, Williams sometimes fails to catch the relevant essentials of work done by Faraday's predecessors, contemporaries, and successors. Nevertheless, this aspect of Williams's work richly merits the prize it has recently been awarded by the U.S. History of Science Society. No other treatment of Faraday has made so apparent the multiple interrelations of his various research subjects—chemical, electrical, metallurgical, pneumatic, acoustical, and others. Nor has any other author so consistently attempted the imaginative reconstructions, essential if Faraday is to be understood, of the conceptual and experimental processes which led to his epochal discoveries, e.g. electromagnetic induction, magneto-optic effects, diamagnetism, and the concept of the electric and magnetic field. The very persistence with which Williams pursues these goals would itself insure the value of his book as a guide to the development of Faraday's science. Williams intends more than a guide, however, and his detailed analyses frequently do not withstand the scrutiny they invite.

The most easily recognised of the difficulties occur in passages which reconstruct the historical context—scientific concepts and problems—within which Faraday's research was done. For anyone, biographer or not, who aims to illuminate scientific development, the attempt is essential. Williams knows this and has read widely in nineteenth-century scientific literature, but his execution is often clumsy nevertheless. For example, in presenting the problems which Faraday's electrochemical researches aimed to resolve, he attributes to Ritter (pp. 228 f.) an experiment in which two water-filled electrolytic cells, connected in series, evolve gases only at the two outermost electrodes, the ones connected directly to the battery. In the context of electrochemical theory before Faraday, Williams supposes, this experiment implied that hydrogen, if truly an element, 'could travel *through* the wire between the [two electrolytic] tubes'. Escape from that interpretation was, he indicates, among the problems Faraday would have to solve. Somehow, Williams has failed to notice that the experiment, as described, presents the same challenge to modern theory that it is supposed to have presented to Faraday. That must be why he did not check Ritter's article once more and discover that the relevant experiments there described are different in a vital respect from the one he reports—one electrode in each cell is surrounded by acid. Or, if the organisation of Williams's narrative were tighter, he might have been led back to Ritter when attempting to describe Faraday's explanation of the putative effect. But, having once posed the gratuitous challenge, he never returns to it.

In its purity that example is far from typical. I doubt that a fully comparable one could be found in the book. But the careless reporting, organisational loose ends, and what occasionally looks like an insufficiently immediate grasp of relevant scientific theories do recur, though usually singly and with more equivocal effects. For example, Williams describes Volta's discovery of the electric current (p. 56) as follows: 'In 1794 he showed that the mere contact of two dissimilar metals could produce an electrostatic charge; when the metallic contact was supplemented by the intervention of a fluid conductor, a continuous circulation of the electric fluid resulted (see Figure I).' Apparently readers are to think of the liquid as placed *between* the two plates of dissimilar metal; that interpretation is enforced by the diagram to which Williams refers.[1] Today's science students do, of course, think of Volta's work in that way, but the view is not that of Volta, of Biot, or of the major Continental school which they inaugurated. For these men the unit cell was the two unlike pieces of metal in direct contact. Liquids intervened only as series conductors between bimetallic cells. The use of liquids was thought to eliminate the electrostatic tensions (we should call them contact potentials) which metallic conductors would have evoked. As an integral part of the electrostatic interpretation of electric-current phenomena the difference is consequential. The electrostatic view made the pile a self-charging battery of Leyden jars, treated external conductors as mere discharge routes, and analysed resistance in terms suggested by Coulomb for the very different phenomena we should call leakage.[1]

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[1] An additional difficulty is illustrated here. Williams's diagrams are sometimes obscure, have no legends, and are insufficiently keyed to text. Only while preparing this review have I realised that the solid black portion of the diagram on p. 56 must be the liquid. If that symbolic coding is transferred to the diagram of the pile on p. 57, then the drawing does not correspond to Volta's. If, as I suppose, solid white is liquid, then the pile is not made up by stacking the individual cells shown on the preceding page. Elsewhere in the book are essential diagrams that neither I nor my students have yet been able to decipher, e.g. those on pp. 152 and 425 as well as the vital redrawn Faraday sketch on p. 157.

That view of electric current phenomena, dominant in many circles until 1820, was still very influential during the years of Faraday's first research in electricity. But it was also under vehement attack from Ampère and a group which rapidly assembled around him. They introduced a radical distinction between electrostatic and current phenomena. The latter were to be investigated exclusively with the galvanometer; electrostatic tension was irrelevant (Ampère emphasised that it dropped to zero as soon as the circuit was closed); the interaction between current-carrying wires was of a different kind from that between static charges. Faraday knew the French literature in which the battle was joined; he corresponded with Ampère who, after Faraday and Davy, is the third major scientist discussed in Williams's book. Yet neither the electrostatic nor the Ampèrian position on these central issues is ever described or discussed. That must be part of the reason Williams can attribute Ohm's law to Faraday (see below) and elsewhere (pp. 224 f., n. 12) entirely miss the major sources of opposition to that law. Accepting it meant the abandonment of one vastly influential aspect of Ampère's work and a retreat to a view more like the Volta-Biot theory which Ampère had, for many scientists, displaced. The same failure of perception enables Williams (pp. 173 f.) to attribute to Ampère an electrostatic explanation of the interaction between currents which, though undoubtedly important to Faraday's discovery of induction, was foreign both to Ampère and the pre-Weberian members of his school. Similar difficulties are, I suspect, to be found in Williams's discussion of the relation between Faraday's electrochemical researches and the earlier work of Grotthus and Davy, but there is no room to develop that point here.

Shortcomings of this sort are, of course, less frequent when Williams deals with Faraday himself as he does in much of the book. But they do occur repeatedly, often in substantive relation to the Whiggish misconstructions illustrated above. For example, Williams discusses (pp. 209 f.) experiments and texts from which he concludes: 'What Faraday had stated was clearly Ohm's law. ... Had Faraday taken the one final step of putting his views into the equation $I = E/R$, he would have provided an enormous stimulus to the progress of electrical science.' But the introduction of 'electromotive *force*' (Williams's term for E in the preceding equation) is gratuitous. Faraday says only that in electromagnetic induction, 'the tendency to generate a current must be directly as [the conducting] power'. In context the passage is plausibly reading as evidence that Faraday thought the 'tendency to produce a current' depended on the nature of the material in which current was induced, an interpretation which gains force from its relation to Faraday's views on the electrotonic state. Nor does Williams pause to wonder whether 'conductibility', as it occurs in this discussion, is at all the notion that develops after the assimilation of Ohm's law though he presents evidence for the continued currency of the leakage concept when discussing Ritchie's experiments three pages later. Whether or not my own conjectural reading is correct, what Williams has given us is neither Faraday's thought nor its context in the contemporary research of others.

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[1] For documentation of a number of these points and for the central idea in the remarks on Ampère, below, I am indebted to a still unpublished paper by Mr Theodore M. Brown. I am grateful to him for permission to anticipate his publication on a few points originally developed in my Princeton University seminar.

Elsewhere Williams reverses the pattern, giving us Faraday's thought so literally and uncritically that its relation to subsequent electrical research is obscured. An important example is the description (pp. 297 f.) of the electrical 'shadow' experiment with which Faraday thought he had refuted Poisson's 'Newtonian' theory of electrostatic induction and established his own polarisation theory in its place. Williams does not inform his readers, and may conceivably be unaware, that Faraday is here dealing with just the sort of phenomenon for which Poisson's theory was devised and with which it was brilliantly successful. As a result, one is deprived of an opportunity to see how Faraday's well-known inability to grasp even the structure of a mathematical argument affected the details of his science. (Williams also obscures this vitally important facet of Faraday's relation with Ampère.) More important, no place is left for Kelvin's proof of the equivalence between Poisson's theory and a modified version of Faraday's though this is the point from which Maxwell started. In the section which follows (pp. 299-315) Williams struggles to eliminate the apparent contradiction, much discussed at the time, between Faraday's insistence that all electrostatic phenomena are ultimately polarisation effects and his use of 'free electricity' to explain how inductive states come into being (p. 313). What Williams has to say on this subject is necessarily speculative—he carefully labels it as such—and way well explain why Faraday never quite confronted the problem squarely. But the attempt to impose consistency deprives the problem of its historical position. The conflict between polarisation and free-charge explanations is deep and consequential. Maxwell inherited it from Faraday; Hertz underlined it in his critique of Maxwell; their successors wrestled with it to the end of the century. Readers might be fascinated to observe its early emergence.

These are, of course, all isolated difficulties torn from their context in a developmental discussion characterised by a richness and scope which my critical examples do not even suggest. Though many others could be given, it would remain the case that Williams must, more often than not, have interpreted Faraday correctly. Nevertheless, until much of this work is done again—a task which the existence of this biography will make far easier—it will remain impossible to tell which points are which. Williams is not a reliable guide to the fine-structure of Faraday's thought, much less to that of nineteenth-century science. And what holds for fine-structure, where texts are present, must hold as well for Williams's explicitly speculative theses about Faraday's main guiding hypotheses. According to Williams, Faraday was committed, throughout most of his productive research career, both to central tenets of German *Naturphilosophie* and to the Boscovichean atom. The former, at least is now increasingly recognised as a vital formative element in nineteenth-century scientific thought. The notion that the two played an essential role in Faraday's development is interesting and might even be correct. But Williams's evidence is extraordinarily thin, and it is far from apparent that any speculations so far-reaching are required for an understanding of Faraday. At the moment they serve to block more obvious lines of research.

The structure of Williams's main argument for Faraday's debt to *Naturphilosophie* is *post hoc ergo propter hoc*. The *Naturphilosophen*, first, and Faraday, second, rejected subtle fluids in favour of natural forces and were further convinced that the various forces of nature were convertible into one another. Those theses were not, however, either in isolation or combination, the exclusive possession of the *Naturphilosophen*. The first had never been lost from view after Newton, and the second had existed at times and places, e.g. eighteenth-century France, where the influence of *Naturphilosophie* is hard to credit. Though I have no idea where Faraday came upon them or whether he invented them for himself, the very limited parallel to *Naturphilosophie* is dubious evidence of influence. And the literary chain by which Williams suggests Faraday may have acquired them seems even weaker. Often it depends upon equating *Naturphilosophie* with Kant's philosophy and alluding to the undoubtedly considerable influence of Kant. The key concept of the interconvertibility of forces is not, however, to be found in Kant, not even in his closest approach to *Naturphilosophie*, the influential *Metaphysische Anfangsgründe der Naturwissenschaft*. No chain connecting Faraday to Kant can have supplied the former with the idea. *Naturphilosophie* itself could have been a source, but there is only one reference under this heading in Williams's index, and he has almost nothing to say about the philosophers who, after Kant, developed and established that view. His most helpful suggestion is a route via Coleridge to Davy to Faraday. But Davy, though he rejected subtle fluids, was not apparently a believer in convertibility. More careful discussion of this whole subject is essential.

Dealing with the Boscovichean atom, Williams feels himself on firmer ground, for in 1844 Faraday did write that 'the atoms of Boscovich appear to me to have a great advantage over the more usual notion' (p. 378). Williams supposes that Faraday had, in fact, embraced the point-atom years earlier—in this case there is no problem of transmission—and deployed it consequentially in his research. His failure to mention the belief before 1844 is, Williams believes, due to his fear of displaying metaphysical commitment. In tracing Faraday's thought, Williams again and again shows how a Boscovichean would have analysed one of Faraday's experiments or presented one of his arguments and then imputes similar thought processes to Faraday.

Here, too, Williams may be right, but I find his arguments even less convincing (partly because they seem so gratuitous—it is not clear that there are profound puzzles in this area) than those he advances for the influence of *Naturphilosophie*. Fear of public metaphysical commitment can scarcely explain the absence of point-atoms from Faraday's *Diary*. In any case, the Boscovichean atom, thoroughly Newtonian in spirit, was not anathema to British scientists as *Naturphilosophie* often was. Faraday's commitment, if significant to his research, should have left more literary traces. More important, the reiterated argument that particular published texts of Faraday's disclose his use of the point-atom depend upon the implicit premise that the only other view he could have held was a crude Daltonianism. (on p. 129 Williams speaks of Faraday's '*experimentum crucis* which could decide between the Daltonian and Boscovichean theories', and compare pp. 123 and 245). On one occasion (p. 251) Williams even appears to attribute 'the coolness of the reception given Faraday's theoretical concepts' to the fact that many chemists were 'still visualizing chemical combination in terms of hooks and eyes'. Williams ought not be held to his catch phrase, but the words are nonetheless symptomatic of a level of argument that does no justice to the elaboration of chemical concepts during the eighteenth and early nineteenth centuries. Repeatedly Williams argues for Boscovichean atomism from passages in which Faraday would seem to have been using more standard chemical ideas. One particularly clear-cut example occurs on pages 278-80. Williams's excellent index, under 'Atomism: Boscovichean', will lead to a number of others.

Possibly these difficulties and others in the analytic passages that bulk so large in Williams's narrative are an inevitable concomitant of the attempt to assault a whole new area of scholarship. In any case, no one can regret the overall result of the attempt. Williams has opened up a vast range of new problems to scholars, has supplied quantities of data for their use, and has proliferated imaginative connections for them to confirm or replace. But, both in its methods and conclusions, the book is an opening up of territory, not a conquest. Unless that is recognised, Williams's *Faraday* could do more harm than good.

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M. Bunge (ed.) *The Critical Approach to Science and Philosophy: Essays in Honor of Karl R. Popper* (New York, Free Press, 1964). Pp. 480.

It is rather unusual in contemporary philosophy to find 'schools of thought' devoted to the exposition and clarification of the ideas of a single thinker. Apart from Wittgenstein and (to a lesser degree) Carnap, Karl Popper is the only recent philosopher in the English-speaking world to have spawned a set of disciples vocal and talented enough to be considered a major force. Whatever lingering doubts there might have been about the veracity of that statement are certainly dispelled by this volume, which makes clear that Popperianism or 'critical rationalism' is a significant movement to be reckoned with. At the same time, this is one of those volumes whose scope and variety makes like uncomfortable for reviewers. Containing twenty-nine essays, this Popper *Festschrift* gives eloquent testimony to the extent of its honouree's influence on the ideas of this generation. The list of contributors reads like an excerpt from an academic *Who's Who:* the philosophers Feigl, Feyerabend, Grünbaum, Hare and Kneale; the logicians Bernays and Curry; the scientists Bohm, Bridgman, Eccles and Frisch; and the historians Geyl, Gombrich and Hayek, to name only a few. Also among the contributors are several of Popper's colleagues and former students: Agassi, Bartley, Nadel, Watkins and Wisdom, for instance. It would be hopeless to attempt to summarise, much less to evaluate, all the essays printed here; accordingly, I shall focus on one of the major threads running through several of them, apologising in advance for neglecting many interesting and important papers and arguments which are peripheral to the main theme.

As its title suggests, this *Festschrift* is designed as a working example of the philosophy of critical rationalism and as an application of that philosophy to many of the classical problems of logic, methodology, metaphysics, epistemology and the philosophy of history. It purports to be a case-study in the 'critical approach' and one reads it eagerly in order to find further enunciation and clarification of the epistemology underlying that catch-phrase. Unfortunately, few contributors ever come to grips with the dual problems of defining and defending that approach.

This failure is certainly not due to lack of effort, for several papers are ostensibly devoted to exploring critical rationalism in hopes of establishing its place on the map of the history of philosophy. Indeed, attempts are made to create a separate niche for critical rationalism and there are claims that it is an unprecedented doctrine which dissolves old problems and poses new ones. Bartley, for example, suggests that Popperianism shifts the epistemological emphasis away from the traditional problem of justifying knowledge and towards a non-justificational philosophy of criticism: 'Philosophers, (Popper) argued, should not demand and search for infallible intellectual authorities, but should instead try to build a philosophical programme for counteracting intellectual error' (p. 21). Bernays makes a similar point; Popper, he tells us, replaces the old question 'How can we justify our knowledge?' with an entirely different one—'How can we hope to detect and eliminate error?' (p. 33). If we are to believe the account here, critical rationalism is the first philosophic doctrine to admit that there are no grounds for justifying our knowledge which does not thereby succumb to relativism and scepticism. The fact that the sceptic can readily show the flaws and false pretensions in any claim to certainty is, so far as critical rationalists are concerned, no reason to believe that all attempts at understanding are on a par. Even if truth is beyond our reach, falsity is not; and our inability to establish the truth of a universal empirical proposition does not preclude our showing its falsity. Hence Popperians conclude that we should abandon all justificatory philosophies, including logical positivism with its emphasis on verification and confirmation, and replace them with a critical philosophy which asserts that although truth cannot be detected, error certainly can. There is still room in such a system for objective truth, which is conceived as the limit we approach as we refute or falsify *seriatim* our theories and conjectures.

Put in this context, it is relatively easy to see the transition stages that have taken Popper from the falsificationist philosophy of science of the *Logik der Forschung* (1934) to his more recent pronouncements on general epistemology. Just as the alleged assymetry between verification and falisfication in science led Popper to opt for the latter, so have the supposed differences between the conclusiveness of proofs and refutations prompted him to adopt his present epistemological standpoint. Thus, it is clear that the critical approach rests, in certain crucial respects, on the viability of Popper's early distinction between the conclusiveness of verifying and falsifying theories. Unless a clear-cut case can be made for unambiguous and determinate falsifications, then critical rationalism is left open to many of the same criticisms which Popper and his colleagues have so forcefully advanced against what they call justificational epistemologies. Given the fact that numerous writers (especially the conventionalists) have severely criticised the concept of unequivocal falsification, it is surprising that no contributor to this volume seriously considers the case against falsificationism. With the exception of the articles by Bunge and Feyerabend, conventionalism and similar philosophies are cavalierly dismissed by appealing to our instinctive predilections for realism.

Even granting that falsification does occur—that, as Munz puts it, there is an 'assymetry between verification and falsification' (p. 89)—it is not clear precisely what Popperians think is falsified in the process of falsification. They concede that individual hypotheses are not singly falsifiable, owing to the necessity of using a number of auxiliary assumptions to derive a prediction from a single hypothesis. Accordingly, they insist that it is always theories or systems of hypotheses which are susceptible of refutation. But it does serious damage to the traditional meaning of terms like 'theory' or 'theoretical system' to assimilate them to the falsifiable antecedent in a scientific explanation or prediction. Suppose for instance, that we are confronted with two theories, one of which implies that the universe is expanding, while the other entails that it remains the same size. We may invoke the familiar red shift as refuting evidence for the latter theory, but in doing so we must be aware that what we are falsifying is not merely the theory about the static size of the universe, but rather the conjunction of that theory with a detailed theory about the behaviour of light. This conjunct can only be called a theory by a highly artificial use of the term. The sorts of things which have traditionally been called theories or systems (e.g. Newtonian mechanics, classical thermodynamics, chemical bonding theory, etc.) cannot be directly falsified; and it is quite misleading to suggest, as Popper and his followers often do, that it is a key property of theories that they are open to definitive refutation.

Neglecting these ambiguities, there are still several difficulties with an epistemology and philosophy of science resting on falsification which are generally ignored by the contributors to this volume. For instance, it is questionable whether scientists, whose activity Popper offers as his epistemological paradigm, are primarily seeking to falsify their theories. Indeed, it has not been established that falsifiability is even the most important desideratum in the decision process of scientists. Furthermore, it is doubtful whether, as Popper seems to claim, the scientific theories which have triumphed historically have always been stronger or more falsifiable than their predecessors. For example, it is almost certainly not the case that the chronological succession of seventeenth-century optical theories or nineteenth-century electrical theories would be accurately reconstructed by an ordering of those theories *vis-à-vis* their degree of falsifiability.[1] The falsifiability of an hypothesis often is a factor in scientific choice; but it is certainly not the only factor and, as Kuhn has argued, probably not even the major one. Obviously, these criticisms do not themselves constitute a case against critical rationalism. They are merely sketches of arguments which have been developed by others in detail elsewhere. It is as unfortunate as it is ironic that this volume takes only scant cognizance of these and other reservations that have been expressed about the critical philosophy.

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[1] Hence I can see little basis for the thesis which Giedymin attributes to Popper to the effect that: 'The history of theoretical science is a series of theories of increasing test ability (strength)' (p. 56).

However, the fact that one may have doubts about the critical approach in general does not diminish one's admiration for many of the specific arguments made by the contributors to the volume. Although most of the papers presuppose the validity of Popper's position, this is not entirely a book written by Popperians solely for Popperians. It is, rather, an attempt to discuss the problems of modern philosophy in the light of certain Popperian motifs which are often profound and always interesting. Perhaps the single area where the critical approach has been of greatest value is with respect to theories of confirmation and inductive strength. Popper's views on confirmation, as well as the amplifications thereof by Watkins and Giedymin here, are effective antidotes to the often pungent prescriptions of inductivists.

Apart from those papers already cited, special mention should be made of Feigl on 'What Hume Might Have Said to Kant', Rescher on Aristotle's modal logic, Grünbaum on reversibility, Bridgman on Mach's principle, Bunge on phenomenological theories and Nadel on the philosophy of history. The volume concludes with a useful bibliography of Popper's writings from 1925 to 1963.

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R. S. Cohen and M. W. Wartofsky (ed.): *Boston Studies in the Philosophy of Science*, Vol. 2 (New York, Humanities Press, $9.75). Pp. xxxiv+475.

This volume consists mainly of papers read between 1962 and 1964 to the Boston Colloquium for the Philosophy of Science: they are for the most part followed by comments. The volume is dedicated as a Festschrift to Philipp Frank, and begins with brief remarks in his honour by some of his students and colleagues together with a brief bibliography of his work.

Like other similar volumes, it lacks any cohesion. Many of the papers are specialised and of interest to only a few. One, by Sir George Thomson, is more general in presenting some of his thoughts on scientific method. It may be of interest in giving the point of view of a distinguished practitioner of the scientific method, but is not likely to provide any new insights. Three of the papers, however, select themselves as being discussions of topics of most central concern. These are those by G. Schlesinger on 'Instantiation and Confirmation'; by Wilfrid Sellars on 'The Identity Approach to the Mind-Body Problem'; and by J. J. C. Smart on 'Conflicting Views about Explanation'.

In his paper Schlesinger considers again the Paradoxes of Confirmation, arising from the logical equivalence of 'All *A* are *B*' and 'All non-*B* are non-*A*', and the fact that instances which confirm the one do not confirm the other. He claims that various solutions, for example, those of von Wright, Hempel and H. G. Alexander do not solve the paradoxes. To show this he expounds a further paradox. If we take the proposition that all birds are black, then it is confirmed by the observation of a black raven. On the other hand a black raven is a non-white non-swan and it would seem to confirm the proposition that all non-white things are non-swans, and its logical equivalent, that all swans are white and its logical consequence, that not all birds are black. Hence the same observation would appear to support both 'All birds are black' and its contradictory 'Not all birds are black'. The paradox seems odd, and Hempel in his comment, suggests certain reservations about its formulation.

Schlesinger suggests that rather than looking at the paradoxes individually, and trying to find solutions to them while accepting the presuppositions on which they are based, we should reconsider the whole notion of confirmation. The direction in which he wants to take us is one that is familiar from such an account of natural necessity as that of Braithwaite. He argues that the proposition to be confirmed cannot be taken in isolation, but must be seen in a context of the whole body of knowledge which we accept and to which the proposition is related. Thus the kind of confirmation required by a proposition will depend on whether the association of two properties makes sense or seems likely in the light of already obtained knowledge. While we may feel the likelihood of there being a connection between property *A* and property *B*, we do not feel the same likelihood of there being a connection between the property non-*B* and the property non-*A*, if indeed these are genuine properties.

I do not think that many either would now or would in the past have disagreed with Schlesinger when he suggests that the paradoxes show that all is not well with the notion of confirmation. His views do not seem as radical as he suggests. It seems to me that a more radical implication of the paradoxes might be a questioning of what is involved in the logical equivalence of 'All *A* are *B*' with 'All non-*B* and non-*A*'. Do these in fact make the same statement?

Sellars's paper has already appeared in the *Review of Metaphysics*. It is an attempt to look at the identity problem from a somewhat different angle. He begins by considering what is meant by identity of universals and by the existence of as yet undiscovered universals. He goes on to apply this to the question of whether raw-feel universals are identical with brain-state universals. He considers that in one sense the two are trivially identical, the sense being that in which raw-feels are states of 'core persons' and only derivatively of the clothed person (the distinction between core persons and clothed persons is that between the brain and nervous system on the one hand and on the other, the brain, etc., as clothed with flesh and bones).

Sellars produces a number of objections to the identity theory in a less trivial form, the most telling of which asks 'How can a property which is in the logical space of neurophysiological states be identical with a property which is not?' But this is not the end of the matter, because a similar question might have been asked about the predicates of chemical and micro-physical theory, at any rate if we consider a possible situation in which chemical theory were as it is at present, and micro-physics were rudimentary. He discusses and rejects the possibility of a reduction of raw-feel universals to brain-state universals which is suggested by such an analogy.

Sellars is undoubtedly discussing a difficult and important problem. But I am unable to judge how successful his arguments are because the arguments are not sufficiently clearly stated. The use of technical terms may sometimes be necessary, but here they seem to obscure the issue. Sellars does try to clarify the notion of identity, but fails partly because he gets involved in a Platonic notion of universals. Similarly much of his argument depends on the notion of logical space, and he takes this notion for granted. Surely before we ask whether brain-state universals and raw-feel universals are in the same or a different logical space, we have to have some criterion for the latter.

Smart's paper is a brief survey of the controversies which have arisen round the questions of whether descriptions are theory-laden, and, connected with this, the relation to each other of different theories, including common-sense, about same subject matter. There follow three comments by Sellars, Putnam, and Feyerabend on these topics. Perhaps the central issue of this particular discussion is the relation of scientific theories to common-sense. Smart sums up the positions of his three representative protagonists as follows: 'According to Nagel common-sense (or much of it) is both true and indispensable. According to Feyerabend it is neither true nor indispensable. According to Sellars it is (methodologically) indispensable but false.'

To give an adequate account of the explanatory functions of scientific theory, it is essential to take up a position on this issue. The most natural position is to see a gap between common-sense and scientific theories. Common-sense may be wrong in certain respects, but it cannot be proved wrong by scientific theory, because the latter depends on common-sense for its basis.

In his comments Feyerabend criticises such a view. He alleges it is methodologically unsound in that it prevents the formulation of alternative theories. For science to progress such a formulation is necessary, and it will only happen if we see that all descriptions including those of common-sense make theoretical assumptions, and we allow the possibility of these assumptions being wrong. But surely no one has denied the possibility of alternative theories in some sense. Feyerabend would, however, not call them real alternatives; to say this seems to require a criterion of what is to count as an alternative theory. Is an emendation, a development, an extension of an old theory, an alternative theory? It would seem not to be if the same concepts are being used. But how do we tell whether the same concepts are being used? The answer seems to be in terms of seeing whether the theories are different. There seems to be considerable circularity in this argument. It would be wrong, of course, to deny that changes of concepts are possible. But Feyerabend has not shown that all changes must be changes of concept.

Is common-sense false and to be emended in the light of, for instance, the theory of relativity? I find it difficult to see common-sense and scientific theories as competing alternatives. A comparison between them is difficult just because the concepts of common-sense are too vague, too imprecise to allow the formulation of much of a scientific theory. I just cannot imagine talking about the desk I am writing on in terms of quantum mechanics except when I am being a scientist, nor can I believe professional scientists do. It seems to me that to claim that common-sense is false is to make ordinary language a great deal more precise than it is, needs to be or ought to be.

Other papers in this volume are: E. McMullin; 'From Matter to Mass', a discussion of the genesis of the concepts of matter and mass; N. R. Hanson, 'Number Theory and Physical Theory', an examination of some of the analogies between mathematics and physics, which may throw light on the way certain philosophers and scientists have failed to understand the differences between mathematics and physics; E. Mendelsohn, 'Physical Models and Physiological Concepts', an attempt to look at the different modes of explanation to be found in nineteenth-century biology, and in particular the contrast between the reductionist claims for biology and the claim that there was need for special biological laws; D. Follesdal, 'Quantification into Causal Contexts', in which Follesdal asks about quantification in connection with contrary-to-fact conditionals, etc, and suggests that the difficulties with it lead to three alternatives; the collapse of modal distinctions, the restrictions on the substitutivity of identity, or the restriction of the use of singular terms, the latter alternative being preferred.

Also included are papers by: H. Marcuse, 'On Science and Phenomenology', a consideration of some of the implications of Husserl's ideas for science; A. Shimony, 'Quantum Physics and the Philosophy of Whitehead', which looks at how well the physical implications of Whitehead's philosophy fit in with more recent quantum theory, as distinct from the earlier forms which Whitehead considers; N. Rudich, 'The Dialectics of Poesis', an exploration of the epistemological status of literature; M. Stock, 'Methodological Reflections on Aquinas and Freud', an examination of the concept of the unconscious mind from a Thomist point of view; and M. Capek, 'The Myth of Frozen Passage', an attempt to show that if any metaphysics deserves to be called a myth, it is the metaphysics of Being rather than that of Becoming.

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Robert Hugh Kargon: *Atomism in England from Hariot to Newton* (Oxford: Clarendon Press, 1966, 42*s*.) Pp. viii+168.

In his Preface the author complains that most histories of intellectual ideas are 'ahistorical', that is, they emphasise the internal development of thought at the expense of the real historical problems of the real men who did the thinking. The complaint is just, and this book goes a long way towards replacing seventeenth-century scientific philosophies in their contemporary context. Professor Kargon describes the influence of two circles of natural philosophers, associated with the Earl of Northumberland and the Duke of Newcastle, whose radical thinking tended to bring atomism into disrepute in the first half of the century. He then describes its revival and evolution in England under the influence of Descartes and Gassend, culminating in its role in Newton's thinking up to the *Principia.* While his 'historical' approach is to be welcomed, he does perhaps allow conceptual issues (for example, regarding different types of atomism and the intellectual grounds for choice between them) to be unduly eclipsed by the historical contexts of schools and sects.

The philosopher of science will probably find of greatest interest Kargon's discussion of Bacon, and his analysis of the different interpretations of Baconianism which were current in the 1660s and '70s. His account of Bacon himself is well-balanced, and he provides a much-needed corrective to most of the older commentators by taking seriously Bacon's scientific development from his early atomism to an explanatory scheme based on the interactions of weightless spiritous substances with gross ponderable matter. This latter Kargon rightly sees as an impressive theory of matter, containing much more than the often-quoted kinetic theory of heat, which itself is more subtle than most commentators have understood. On a purely historical point, which nevertheless has its conceptual importance, Kargon seems a good deal more confident in his dating of some of Bacon's writings (p. 44) than, for example, F. H. Anderson in his *Francis Bacon: His Career and his Thought* (1962), but without disclosing the source of this confidence.

Kargon's discussion of the Baconian reaction of the 1660s against 'hypothetical physics' is full of interest. He shows how some natural philosophers interpreted Bacon as an extreme empiric and eschewed all hypotheses, while others, principally Boyle, attempted to safeguard empiricism rather by illustrating and confirming (but not *proving*) the current mechanical hypotheses. Barrow, followed by Newton, had yet a third view of the Baconian tradition, which came closer to Bacon's own insistence that the inference from experiment to theory can be *certain*. Barrow and Newton claimed to arrive at certainty of inference by the use of mathematics in conjunction with experience. It is good to find Kargon placing renewed emphasis on the *inductivism* of Newton's intentions. More patterns of scientific inference were dreamed of in the seventeenth century than in some more recent philosophies of science.

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Recent Publications

RECENT PUBLICATIONS ON THE PHILOSOPHY OF SCIENCE

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J. W. Addison, J. Henkin and A. Tarski: *The Theory of Models.* Proceedings of the 1963 International Symposium at Berkeley. (Amsterdam, North-Holland Publishing Co. 1965. 130*s.*) pp. xv+494

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Abstracts

Philosophy of Science, 33 (1966), No. 1

R. M. Martin, 'On Theoretical Constructs and Ramsey Constants'

The method of Ramsey sentences has been proposed for handling theoretical constructs within a scientific system. Essentially it consists of constructing a certain 'monolithic' sentence for an entire theory. In this present paper several improvements are suggested which help to overcome some of the awkward features of the method. In particular we have here *many* Ramsey sentences rather than just one, each erstwhile primitive theoretical term giving rise to a Ramsey sentence. Such a sentence in effect defines what we call a *Ramsey constant*. Using Ramsey constants, we attempt to improve the method in important logical and semantical respects. It is suggested also that such constants are of interest for the philosophy of mathematics.

Patrick Suppes, 'The Probabilistic Argument for a Non-Classical Logic of Quantum Mechanics'

The aim of this paper is to state the single most powerful argument for use of a non-classical logic in quantum mechanics. In outline the argument is the following. The working logic of a science is the logic of the events and propositions to which probabilities are assigned. A probability should be assigned to every element of the algebra of events. In the case of quantum mechanics probabilities may be assigned to events but not, without restriction, to the conjunction of two events. The conclusion is that the working logic of quantum mechanics is not classical. The nature of the logic that is appropriate for quantum mechanics is examined.

Joseph D. Sneed, 'Von Neumann's Argument for the Projection Postulate'

Much of the recent discussion of problematic aspects of quantum-mechanical measurement centres around that feature of quantum theory which is called 'the projection postulate'. This is roughly the claim that a change of a certain sort occurs in the state of a physical system when a measurement is made on the system. In this paper an argument for the projection postulate due to von Neumann is considered. Attention is focused on trying to provide an understanding of the notion of 'the state of a physical system' which is compatible with the argument von Neumann offers. An attempt is made to formulate the argument in terms of an objectivistic interpretation of probability concepts. It is seen that such an interpretation does not provide a suitable way of understanding the argument. An attempt is made to illustrate the source of this failure in terms of a non-quantum-mechanical example.

Theodore Mischel, 'Pragmatic Aspects of Explanation'

How can reasons explain actions? What is the force of 'because' in 'He did this because . . .' followed by a statement of the agent's intentions? The answer involves some concept of what can count as explanation, and the history of science indicates that the acceptability of explanations depends, in part, on a scientific community which has decided to pursue its inquiries in one direction rather than another. The first part of this paper examines this pragmatic aspect of explanations; the second part draws on this examination in the hope of elucidating the way reasons explain actions. The possibility of eliminating our ordinary, 'purposive' explanations of actions in favour of some 'mechanistic', neuro-physiological, account is then considered.

Philosophy of Science, 33 (1966), No. 2

Laird Addis, 'Freedom and the Marxist Philosophy of History'

Many believe that the Marxist philosophy of history entails that man is not free in a sense in which it seems obvious that he is. In particular it is held to be (1) materialistic, (2) holistic, (3) economistic, and (4) fatalistic. It is claimed, in short, that since the Marxist philosophy of history has these features, man is not capable of shaping his own (social) destiny if it is true. I show for each of these features either that it does not entail what it is believed to entail or that it is not correctly attributed to the Marxist philosophy of history.

Joseph Margolis, 'Objectivism and Interactionism'

The views of linguistic analysts and objectivists are explored with regard to the question of interactionism. It is argued that the admission of a logical difference between explanation by cause and explanation by motive cannot disqualify causal explanations of human action, cannot be construed as challenging the competence of science, and cannot count against interactionism. It is also argued that objectivist programmes for eliminating mentalistic concepts either implicitly admit interactionism or cannot distinguish relevantly between interactionism and parallelism.

Paul Diesing, 'Objectivism vs. Subjectivism in the Social Sciences'

Recent developments in social science methods have made most of the objectivism-subjectivism arguments in the philosophy of social science obsolete. Developments in experimental methods have made possible a behaviouristic treatment of everything cherished as important in human action by the subjectivists; developments in computer and mathematical models have made possible a type of theory which carries out the programme of the subjectivists but is not vulnerable to the arguments of the objectivists. What remains of the philosophical argument are two types of theory (for example, game theory and learning theory) which are both useful, both scientific, and frequently equivalent. Choice between them by scientists can be made on empirical grounds rather than on the grounds developed in the philosophical controversy.

S. Cannavo, 'Extensionality and Randomness in Probability Sequences'

The charge that the limit-frequency theory of probability is inconsistent due to incompatibility between the required features of randomness and limit convergence is inapplicable when probability sequences are taken to be empirically (i.e. extensionally) generated, as they must be on a strictly empirical conception of probability.

All past attempts to meet this charge by formulating constructive definitions of randonmess that would still allow for a demonstrable limit-convergence have, in their exclusive concern with logically (i.e. intensionally) prescribed sequences, left the logic of extensional classes essentially untouched. In the light of a strict differentiation between intensional and extensional classes a generalised approach is possible under which several closely connected senses of randomness, i.e. the formal, material, restricted and unrestricted senses may be easily distinguished and related to the notion of relevance.

Michael T. Ghiselin, 'On Semantic Pitfalls of Biological Adaptation'

'Adaptation' has several meanings which have often been confused, including relations, processes, states, and intrinsic properties. It is used in comparative and historical contexts. 'Adaptation' and 'environment' may designate probabilistic concepts. Recognition of these points refutes arguments for the notions that: (1) all organisms are perfectly adapted; (2) organisms cannot be ill-adapted and survive or well-adapted and die; (3) adaptation is necessarily relative to the environment; (4) change in environment is necessary for evolution; (5) preadaptation implies teleology. Such notions are associated with metaphysical ideas, and may affect the thinking of biologists.

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CALCUTTA

Science and Reality*

by MICHAEL POLANYI

The purpose of this essay is to re-introduce a conception which, having served for two millennia as a guide to the understanding of nature, has been repudiated by the modern interpretation of science. I am speaking of the conception of reality. Rarely will you find it taught today, that the purpose of science is to discover the hidden reality underlying the facts of nature. The modern ideal of science is to establish a precise mathematical relationship between the data without acknowledging that if such relationships are of interest to science, it is because they tell us that we have hit upon a feature of reality. My purpose is to bring back the idea of reality and place it at the centre of a theory of scientific enquiry.

The resurrected idea of reality will, admittedly, look different from its departed ancestor. Instead of being the clear and firm ground underlying all appearances, it will turn out to be known only vaguely, with an unlimited range of unspecifiable expectations attached to it.

It is common knowledge that Copernicus overthrew the ancient view that the sun and the planets go round the Earth and that he established instead a system in which it is the sun that is the centre around which all planets are circling, while the Earth itself goes round the sun as one of the planets. But we do not see it recognised that in the way Copernicus interpreted this discovery, he and his followers established the metaphysical grounds of modern science. We cannot find this recognised, since these grounds of science are predominantly contested today.

The great conflict between the Copernicans and their opponents, culminating in the prosecution of Galileo by the Roman hierarchy, is well remembered. It should be clear also that the conflict was entirely about the question, whether the heliocentric system was real. Copernicus and his followers claimed that their system was a real image of the sun with the planets circling around it; their opponents affirmed that it was no more than a novel computing device.

For thirty years Copernicus hesitated to publish his theory, largely because he did not dare to oppose the teachings of Aristotle by claiming that the heliocentric system he had set up was real. Two years before the publication of his book is 1543, the protestant cleric Osiander responded to preliminary publications of the Copernican system by a letter pressing Copernicus to acknowledge that science can only produce hypotheses representing the phenomena without claiming to be true. Later, Osiander succeeded in introducing an Address to the Reader into the published book of Copernicus denying once more the reality of the Copernican system. The issue was still the same, more than half a century later, in Kepler's defence of Tycho Brahe against his critic Ursus, and the same again when Galileo confronted Cardinal Bellarmine and afterwards Pope Urban VIII.

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* Received 12.i.67

The conflict was settled, at least for secular opinion, when the Copernican system was confirmed by Newton. Copernicus and his followers were recognised then to have been right; and for the two following centuries their steadfastness in defending science was unquestioningly honoured among modern educated people.

I myself was still brought up on these sentiments; but by that time some eminent writers were already throwing cold water on them. The positivist critique of science, initiated by Ernst Mach,[1] and vigorously supported by Henri Poincaré,[2] declared that the claim which Copernicus, Kepler and Galileo so bitterly defended was illusory. This radical positivism taught that science consisted merely in establishing functional relations between the data observed by our senses and that any claim that went beyond this was undemonstrable. A reality underlying mathematical relations between observed facts was a metaphysical conception, without tangible content.

During the past half century these formulations of positivism have been first sharpened into logical positivism, which claimed to establish strict criteria for the meaning and validity of all empirical statements. But logical positivism, after reaching its highest prestige in the forties, presently declined for its aims proved unattainable. Its theories were softened down then by a series of qualifications, which amounted to abandoning any attempt at establishing a formal criterion of the meaning and validity of a scientific statement. The rise of analytic philosophy confirmed this abdication by abandoning the critique of science. Thus we are left today without any accepted theory of the nature and justification of natural science.

---

[1] Ernst Mach, *Die Mechanik in ihrer Entwicklung* (1883).

[2] Henri Poincaré, *Science et Hypothese*, Paris (1902), pp. 140-1, and Henri Poincaré, *La Valeur de la Science*, Paris (1914), pp. 271-4. In the latter book Poincaré defends himself against being taken to reject Galileo's affirmation that the Copernican system was true. He identifies convenience with coherence and ascribes to coherence the value of a greater truth than that which Galileo had claimed.

There has been sharp opposition to the positivist movement by individual authors, among them Planck[1] and Einstein,[2] as well as the great historian of science, Alexandre Koyré;[3] but these authors supplied no statement of the true metaphysical foundations of science. This is the situation in which I shall examine the Copernican's claim that the heliocentric system was a true account of reality. And I shall show that in their conception of reality we can find the actual grounds on which science has rested ever since Copernicus established modern science on these grounds.

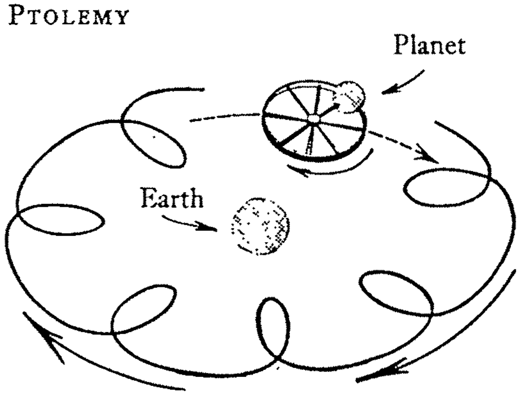


FIG. 1.

In order to explain what made Copernicus feel that the new system he proposed was real, I shall give an outline of an important feature of the old system and show the way the new system dealt with this feature.

When you watch a planet night after night you see its position shifting through the firmament of fixed stars. Seen from a point in the northern hemisphere, it moves predominantly from west to east, but does not move steadily. It speeds up, slows down, retraces its path and continues eastwards again. It passes through these loops at regular intervals. The Ptolemaic system explained these loopings by assuming that the planet—instead of simply moving round its orbit—is carried round this orbit, as it were, on the edge of a wheel. While the wheel moves round the planet's orbit, it keeps turning around its own axle and thus—when seen from the Earth—produces the loops in the planet's journey round its orbit. Such wheels are known as epicycles, and the particular type I have described, are the major epicycles. (Fig. 1.) The path along which the epicycles are carried round is known as the deferent.

---

[1] See footnote to p. 17.

[2] Albert Einstein, Biographical Notes in *Albert Einstein Philosopher-Scientist*, ed. P. A. Schilpp, New York (1949), p. 49, writes about Ostwald and Mach:

> 'The antipathy of these scholars towards atomic theory can indubitably be traced back to their positivistic philosophical attitude. This is an interesting example of the fact that even scholars of audacious spirit and fine instinct can be obstructed in the interpretation of facts by philosophical prejudices. The prejudice—which has by no means died out in the meantime—consists in the faith that facts by themselves can and should yield scientific knowledge without free conceptual construction.'

[3] Alexandre Koyré, *Les Origines de la Science Moderne*, Diogene, October 1956, no. 16, attacks positivism for denying that science has knowledge of reality.

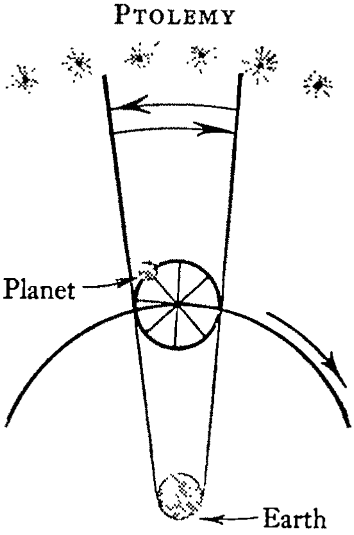


FIG. 2.

The effect of epicyclic motion can be analysed more closely by assuming that the planet has for a moment discontinued its main eastward motion around its orbit. Viewed from the Earth at rest, the image of the planet among the fixed stars would oscillate then at the rate of its passage around the epicycle. (Fig. 2.) Add then to the oscillation once more the orbital motion from west to east and you obtain the looping as observed from the Earth. (Fig. 3.)

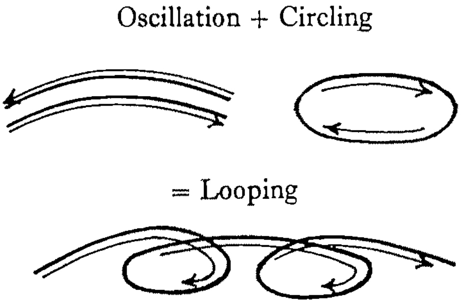


FIG. 3.

The fundamental idea of Copernicus was that instead of having the planets circling on epicyclical wheels, we can get the same effect by putting the Earth into a circular motion round the sun. You can see how this works if you imagine once more, a planet arrested for a moment in its orbital motion and then trace its image against the firmament of stars. The image oscillates in the same way as the planet was seen to oscillate in the Ptolemaic system. (Fig. 4.) And if we again add to this oscillation the eastward orbital motion of the planet, we see arising the observed looping. But the loops are now mere illusions, without any underlying epicyclical motion.

COPERNICUS

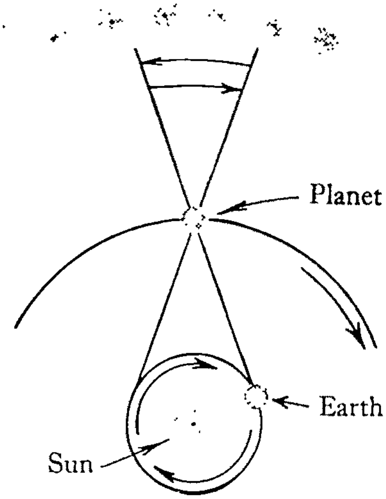


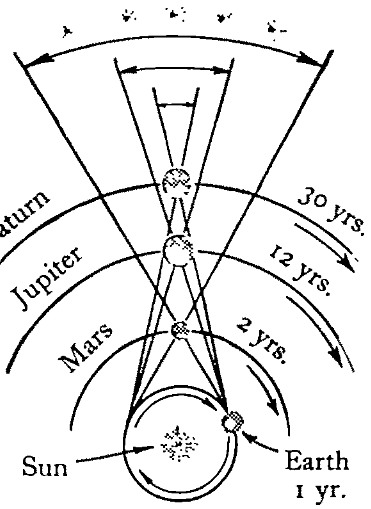
FIG. 4.

Let me add some further features of the Ptolemaic system which were known before Copernicus. *First*, all the wheels representing major epicycles were seen to go round at the same rate, and this common period of the epicycles was found to equal *one year*. Hence a planet like Mars, with an orbit of about two years, passes through one loop in rounding its orbit; while Jupiter, which completes its orbit in twelve years, passes through eleven loops, and Saturn, taking about thirty years to go round, makes twenty-nine loops on its way.[1] *Second*, it was observed and noted that the apparent *size* of the loops decreases with the increasing length of orbital periods from Mars, through Jupiter to Saturn.

---

[1] In reply to an enquiry, Professor S. Sambursky of the Hebrew University of Jerusalem wrote as follows: 'The Almagest in fact does mention the relevant figures for the epicyclic loops of the planets and their amplitudes (in Book IX, ch. 3, and Book XII), but Ptolemy of course fails to interpret these data, and their numerical relations are mere coincidences for him.' The knowledge of this relationship is clearly taken for granted by Rheticus in the *Oratio Prima* (1540), but I could find no mention of it in *De Revolutionibus*.

To these two observations about the major epicycles, we may add as a *third* point a widespread speculative idea about the size of the orbits around which the planets and the sun were thought to circle round the Earth. In order to understand, however roughly, the grounds for this idea, suppose for a moment that all these circling bodies go round their orbit at the same speed, so that the time each takes to get round its orbit would be proportional to the radius of its orbit. The orbit of Saturn should then be the largest, Jupiter's smaller and Mars's smaller still. More will be said later about the derivation of this sequence.



COPERNICUS

FIG. 5.

Turning now to the heliocentric theory of Copernicus, we find that the three main features of the Ptolemaic system fit in well with the new theory. Firstly, once epicycles are replaced by an illusory oscillation corresponding to the annual motion of the Earth around the sun, all these oscillations must occur at the same rate equalling one year. Secondly, we can see that, in the Copernican system, differences in the observed angles of oscillations imply that the planets are at different distances from the sun. (Fig. 5.) The observed decrease in the angles of oscillations in the sequence of Mars, Jupiter and Saturn yields then the conclusion that the orbital radii of these planets increase in this order, which accords with the ancient surmise, that the size of planetary orbits steadily increases as the orbital period gets longer.

I have so far based the transformation of the Ptolemaic system into Copernican terms on the three outer planets known to Copernicus, and must now introduce the two inner planets Venus and Mercury. When applied to the inner planets, the meaning of Fig. 1 is inverted. The period of the epicycle becomes the orbital period of the planet, and the period of the deferent becomes the period of the planet's oscillation, i.e. of its illusory motion due to the Earth's annual passage round the sun. For Venus and Mercury the amplitude of these oscillations is found to fall in line with that of the outer planets, and accordingly the calculated solar distance of Venus comes out smaller than that of Mars and the solar distance of Mercury smaller still than that of Venus. All three features of the Ptolemaic system are thus confirmed for the inner planets, when their deferents are taken as equivalent to the major epicycles of the outer planets.

Looking at this result with the eyes of a modern scientist, I would say that the most striking achievement of the new theory was that it explained the strange fact that the observed periods of loopings were all identical and equal to one terrestrial year. But these coincidences strike us as strange only because they are inexplicable in terms of mechanics. Copernicus had no idea that planetary motions were due to mechanical causes and hence his explanation of these coincidences, which is a triumph to our way of thinking, made no such impression on him. To us the very fact that the Copernican system eliminates the major epicycles by which Ptolemy explained the loopings of the planets, is a great achievement. But Copernicus does not explicitly mention this, presumably because he used epicycles readily in other parts of his system, and so they were nothing strange to him.

To the scientist today the mere prediction of the relative orbital radii of the planets is a great achievement of the Copernican system. But Copernicus sees it mainly in relation to the sequence of planetary distances derived from a mistaken theory.

His theory of this sequence is stated in *De Revolutionibus*, book 1, chapter 10, in two passages, one at the beginning the other later. I quote the second passage first for it is more explicit.

> Quapropter prima ratione salve manente, nemo enim conventiorem allegabit, quam ut magnitudinem orbium multitudo temporis metiatur, ordo sphaerum sequitur in hunc modum a summo capiens initium . . . (Capiens || capientes MS.) (*De Revolutionibus*, ed. Thoruni 1873, L. Prowe, E. de Losson, Boethke, Hagemann, p. 28.)

This reads in translation:

> Therefore, if the first law is still safe—for no one will bring forward a better one than that, the magnitude of the orbital circles should be measured by the

magnitude of time—then the order of the planets will follow in this way . . . (C. G. Wallis, *Great Books of the Western World*, vol. 16, Chicago (1952)).

To say that the radii are 'measured by the magnitude of time' is to assert that the linear velocity of all planets is the same.[1] The words at the opening of the chapter appear to bear this out.

> Errantium vero seriem penes revolutionum suarum magnitudinem accipere voluisse priscos philosophos videmus, assumpta ratione, quod aequali celeritate delatorum, quae longius distant, tardius ferri videntur, ut apud Euclidem in Opticis demonstratur.

'aequali celeritate delatorum', i.e. 'objects moving with equal speed', could conceivably be construed as a hypothetical statement by Euclid, but as it fits in with the subsequent description of the planets, this description appears to be its meaning.

Copernicus rejoices that the sequence of the planets computed by him conforms to the law derived from the equality of linear speeds. But, of course, the same sequence is obtained by Kepler's Third Law under which it is the *squares* of times that are proportional to the *cube* of the radii. The relation from which Copernicus derived the sequence of the planets was grossly erroneous. Could he have failed to notice the error? I think he knew his mistake.

In his *Commentariolus*, composed a number of years before *De Revolutionibus*, Copernicus presents the computation of the relative radii for the three outer planets as follows:

> Saturn's deferent revolves in 30 years, Jupiter's in 12 years, and that of Mars in 29 months; it is as though the size of the circles delayed the revolutions. For if the radius of the great circle is divided into 25 units, the radius of Mars' deferent will be 38 units, Jupiter's 130⅕ and Saturn's 230⅙. By 'radius of the deferent' I mean the distance from the centre of the deferent to the centre of the first epicycle. (*Three Copernican Treatises*, translated with introduction by E. Rose, 2nd edn., Dover Publications, New York, p. 74. Note that the 'great circle' means the orbit of the Earth.)

The ratios of the figures 230⅙ : 130 : 38 : 25 agree within about 2 per cent with Kepler's Third Law, but they greatly differ from the corresponding ratios of the orbital periods, which are 29.5 : 12 : 2 : 1. Comparing the ratios for Saturn and Earth, that is the ratio 230⅙ : 25 with the ratio 29.5 : 1, the discrepancy between them amounts to about *a factor three*, and when we extend these ratios to include Mercury, the deviation from proportionality rises ***nearly to a factor five.*** I believe it was these deviations from proportionality that Copernicus had in mind when he wrote: 'It is as though the size of the circles delayed the revolutions.' This delay would, e.g. have to reduce the speed of Saturn to close on one-fifth of the value predicted by Copernicus in order to make its actual speed agree with the observed speed of Mercury. This could not have gone unnoticed by Copernicus.

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[1] J. F. Dobson and S. Brodetsky, Nicolaus Copernicus, De Revolutionibus, Preface and book 1, Roy. Astron. Soc. 1947, translate 'prima ratione salve manente' as 'given the above view' which seems better than 'if the first law is still safe'. They translate 'quam ut magnitudinem orbium multitudo temporis metiatur' as 'that the periodic times are proportional to the sizes of the orbits' which is perhaps too free, even though substantially right.

Consultations with historians have failed to reveal a place in literature mentioning this error; it seems to have been overlooked. My point in bringing it up is to suggest that Copernicus disregarded this discrepancy, because his conception of physical magnitudes in the celestial domain was but vague and that this vagueness must have been worsened still further by his knowledge of this discrepancy. Seen against this background, his prediction of the radii could not have for him the solidity which it has for the modern scientist, for whom most other details of the Copernican system are, by contrast, mere fictions.

What remained then for Copernicus to convince him of the reality of his system? We can sum it up in one sentence. He had succeeded in explaining planetary loopings by a theory which, when introducing the actually observed periods and amplitudes of oscillations, predicts a plausible sequence of orbital radii. It was this achievement to which Copernicus fastened his hopes during thirty years of travail and on the grounds of which he and his followers claimed, against bitter opposition, that the heliocentric system was real.

In his preface addressed to Pope Paul III Copernicus writes that he has

> at last discovered that, if the motions of the rest of the planets be brought into relation with the circulation of the Earth and be reckoned in proportion to the orbit of each planet, not only do their phenomena presently ensue, but the orders and magnitudes of all stars and spheres, nay the heavens themselves, become so bound together that nothing in any part thereof could be moved from its place without producing confusion of all the other parts and of the Universe as a whole.

Everything is now bound together, he claims, and this is a sign that the system is real.[1]

But why did this claim evoke such protest among his contemporaries, particularly by the clergy? The objection was not raised primarily in defence of the bible, but of the medieval philosophy held by clerics and lay scholars alike, since the day of Aquinas 300 years before. The philosophic view which clerics from Osiander to Bellarmine and lay scholars like Melanchton defended, goes back to Aristotle. It assumed that all basic features of the universe can be derived from necessary first principles; for example, the perfection of the universe required that the course of all heavenly bodies be represented by steady circular motions. Such views excluded the possibility of discovering basic features of nature by the empirical observations of the astronomer; any theory established by representing astronomical observations could only be regarded as a mere computing device, and this applied as much to the Ptolemaic system as to that of Copernicus. Only philosophy was competent to arrive at an understanding of essential reality in nature.

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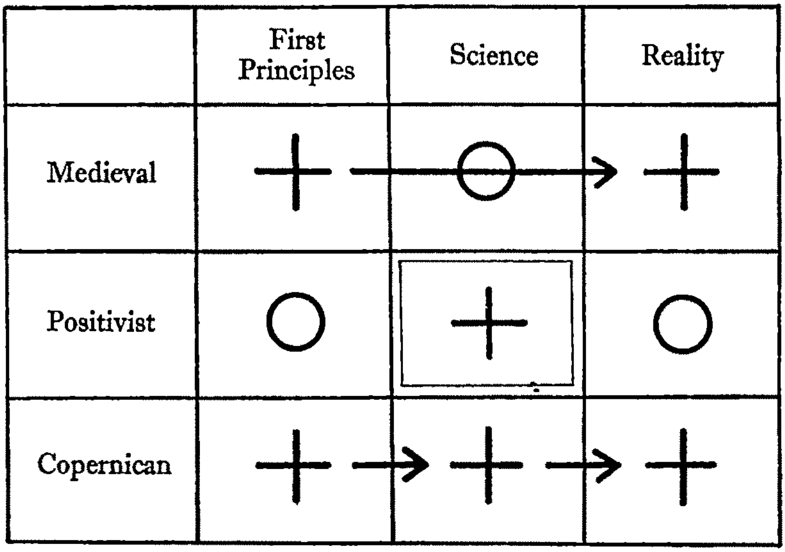
[1] 'The motion of the Earth, therefore, suffices to explain so many apparent inequalities in the heavens' wrote Copernicus in *Commentariolus*. That coherence is a token of reality, is expressed by Rheticus in his *Narratio Prima* (1540) by the words: 'So wise is our maker, that each of his works has not one use, but two or three or often more.'

Centuries later the positivists declared once more that science can say nothing about ultimate reality, but theirs was a very different reason, namely that they though any such claim to be meaningless. Their purpose was not to preserve to philosophy the competence for metaphysical theories, but on the contrary, to purify science from making any such empty claims.

The meaning of these two different attacks on Copernicanism, the medieval and the positivist attacks, and the position of Copernicus himself, can be illustrated by a diagram as follows.

Three Views of Science



	First Principles	Science	Reality
Medieval	+	→ ⊖ →	+
Positivist	○	$\boxed{+}$	○
Copernican	+ →	+ →	+

Note, the term 'science' is used here in the modern sense as applying to astronomy.

(1) In the medieval position first principles bear directly on reality, while excluding science from such bearing. (2) The positivist movement is shown isolating science on the one hand from any extra-scientific first principles and on the other hand from reality, since neither of these is recognised. Science, regarded merely as a convenient summary of given facts, is strictly self-contained. (3) Copernicanism is shown, thirdly, claiming to apply basic principles *through empirical science* for discovering reality.

Copernicus did not contest the competence of philosophy to arrive at necessary conclusions about the nature of things. When Osiander reminded him that his astronomy fails to explain the motions of the planets,[1] he must have agreed that these motions could be understood only from first principles and not from his astronomy. His strict adherence to the steady circular motion of heavenly bodies, which made his system inordinately complicated and clumsy, showed him to be basically an Aristotelian. But he was irresistibly compelled by the appearance of his own system to claim that this particular feature of the celestial order, though derived essentially from experience, was true and real. Thus did he make for the first time the metaphysical claim that science can discover new knowledge about fundamental reality and thus did this claim eventually triumph in the Copernican revolution.

Such is the claim of science to know reality, that positivism disowned in our time; and it is this same metaphysical claim, now widely discredited, that I want to re-establish today.

Let me start by asking, what Copernicus meant by saying, that his system was real? What had he actually in mind when believing that the planets really circle the sun? We shall find a clue to this question if we first look at the more active form of this belief which Kepler and Galileo manifested when undertaking their enquiries. They testified to their belief that the Copernician system was real, by relying on it as a guide to discovery.

I shall show this for Kepler. His Third Law, discovered seventy-six years after the death of Copernicus, developed the feature of the heliocentric system, which Copernicus had mentioned as its most striking harmony, namely the fact that all six planets recede steadily further from the sun in the sequence of their longer orbital periods. Kepler lent precision to this relationship by showing that the cube of solar distances is proportional to the square of the orbital periods. His other great discovery—

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[1] In his Address to the Reader prefacing the *De Revolutionibus*, Osiander says of the celestial movements, that the astronomer '. . . cannot by any line of reasoning reach the true causes of these movements . . .'

ten years earlier—of his First and Second Laws, was in some sense a departure from Copernicus. It broke away from the doctrine of steady circular planetary motions and introduced instead an elliptic path and a law of variable velocities related to the ellipse. Yet this elliptic path, with the sun in one focus of it, was firmly tied to the heliocentric system. It could not have been discovered from Ptolemy's image of the planetary system.

I would not hesitate to say that these discoveries *proved* the reality of the Copernicus system, but this is only because I know that Newton discovered towards the end of the same century that these three laws of Kepler were expressions of the law of universal gravitation. At the time at which Kepler put his laws forward, mixed up with a number of other numerical rules that were to prove fallacious, the effect of his three laws was not widely convincing; Galileo himself was unimpressed by them. But for the moment I can set these questions aside, for I am only trying to understand what Kepler and Galileo themselves believed about the Copernican system, when they relied on their conviction that it was real and thus a proper guide to their enquiries.

At first glance it seems easy to see what happened in Kepler's and Galileo's minds. Relying on the reality of the Copernican system, they recognised the presence of *problems*, which by many years of labour they proved to have been fruitful. But this leaves open the question, how the Copernican system could indicate to them *good* problems that were not visible in the Ptolemaic system.

We meet here a general issue, which to my knowledge, has never been systematically examined. We must ask: What is a problem? Not the kind of problem set to students of mathematics, or to chemists in practical classes, but a scientific problem the solution of which is unknown, and on which the scientist may yet embark with a reasonable hope of discovering something that is new and that will prove also worth the labour and expense of the search.

I would answer that to have such a problem, a good problem, is to surmise the presence of something hidden, and yet possibly accessible, lying in a certain direction. Problems are evoked in the imagination by circumstances suspected to be clues to something hidden; and when the problem is solved, these clues are seen to form part of that which is discovered, or at least to be proper antecedents of it. Thus the clues to a problem anticipate aspects of a future discovery and guide the questing mind to make the discovery.

We may say then that Kepler's conviction that the Copernican system was real, was expressed by his belief that its image anticipated aspects of something hidden and possibly accessible by an enquiry in a certain direction. And we may add that he *confirmed* these anticipations when, by following their guidance, he discovered his three laws.

Nor was this all. For in their turn, Kepler's discoveries raised new problems in Newton's mind, insofar as they anticipated aspects of the still hidden laws of gravitation, which Newton was to discover. Thus Newton was guided still further by a belief in the reality of the Copernican system.

The confidence which the followers of Copernicus placed in the reality of the Copernican system consisted, then, in surmising still hidden implications in it, as were suggested to them by certain features of the system. Their belief in the system's reality was an act of their imagination that spurred and guided them to discovery.

Let us take stock of the position we have reached so far. In the history of the Copernican Revolution we have found it possible to discriminate the explicit statements of a theory from its anticipatory powers. The celestial time-table set out by Copernicus was not markedly different from that of Ptolemy. Close on to a century following the death of Copernicus, all efforts to discriminate convincingly between the two systems on the grounds of their observable quantitative predictions had failed. While the discoveries of Kepler and Galileo based on the heliocentric system greatly increased its plausibility and eventually convinced most astronomers, the general effect, judged for example by the critical responses of Bacon or Milton, was far from conclusive. Yet all this time the theory of Copernicus was exercising heuristic powers absent in the system of Ptolemy. We are faced with the question then how one of two theoretical systems, having virtually the same explicit content, could vastly exceed the other in its anticipations.

In a way I have given the answer to this question in the anticipatory suggestions offered by the Copernican system to the followers of Copernicus. Its anticipatory powers lay in the new image by which Copernicus recast the content of the Ptolemaic system. It is in the *appearance* of the new system that its immense superiority lay; it is this image that made the Copernican revolution.

I am drawing here a distinction which will prove decisive. I distinguish between the precise predictive content of a mathematical theory consisting in a functional relation of measured variables and a meaning of the theory which goes beyond this. While the functional relations remain the same, the surplus of meaning which goes beyond them may vary, as manifested in this case by the appearance of the theory.

The way this may come about can be illustrated from everyday life. Suppose we have a list of all the towns of England, each with its precise longitude and latitude, and the number of its inhabitants, and we now represent these data in a map, each town being marked by a circle corresponding to its size. The mapping of the list adds no new data to it, yet it conveys a far deeper understanding of these data. It reveals, for example, the way the population is distributed through the country and suggests questions about the reasons of physical geography and history which will account for this distribution. The map will guide the imagination to enter on fruitful enquiries to which the original list would leave us blind.

E. M. Forster has shown a similar difference between two kinds of characters in a novel. There are *flat* characters whose behaviour can be precisely predicted and *round* characters which develop creatively; the latter, says Forster, are more real and hence have the power convincingly to surprise us. By bearing on reality, scientific theories too have the power convincingly to surprise us.

But here I must enter a warning. The distinction between explicit content and informal heuristic powers is profound, but not absolute. No mathematical theory means anything except as understood by him who applies it, and such an act of understanding and applying is no explicit operation; it is necessarily informal. Indeed, great discoveries can be made by merely finding novel instances to which an accepted theory applies. For example, Van t'Hoff's demonstration that the mass action law of chemistry was an instance of the Second Law of thermodynamics was a fundamental discovery. When I speak of the explicit content of a theory, I refer to such applications of it which are fairly obvious, and I distinguish these from *a yet indeterminate meaning of a theory that may be revealed only much later, through a scientist's imagination.*

But was Copernicus himself, when expressing his belief in the reality of his system, in fact asserting that it had anticipatory powers, which the Ptolemaic system had not?

It is not clear how anticipatory powers can be known at all, apart from relying on them as clues to an enquiry. Copernicus obviously did not know that his system represented an aspect of Kepler's laws and of Newton's theory of general gravitation; indeed, being wedded to an explanation of the planetary system in terms of steady circular motions, he would have strictly rejected Kepler's laws and Newton's theory based on these laws. Yet his belief that his system was real, was basically akin to that of his great successors. For he saw the essence of his system in those features of it which were to serve as clues to the problems of Kepler and Newton. He saw in the increase of orbital periods with increasing solar distance the characteristic feature of a system in which the sun centrally controls the order of planets, and this is the feature on which Kepler and Newton were to build their discoveries.

But there is actually a more general kinship between the commitment of Copernicus to his belief that his system was real and that of his followers relying on it for their problems. What Copernicus believed of this system was what we all mean by saying that a thing is real and not a mere figment of the mind. What we mean is that the thing will not dissolve like a dream, but that, in some ways it will yet manifest its existence, inexhaustibly, in the future. For it is there, whether we believe it or not, independently of us, and hence never fully predictable in its consequences. The anticipatory powers which Kepler, Galileo and Newton revealed in the heliocentric system, were as many particulars of ***the general anticipations that are intrinsic to any belief in reality.***

This defines reality and truth. If anything is believed to be capable of a largely indeterminate range of future manifestations, it is thus believed to be *real.* A statement about nature is believed to be true if it is believed to disclose an aspect of something real in nature. A true physical theory is therefore believed to be no mere mathematical relation between observed data, but to represent an aspect of reality, which may yet manifest itself inexhaustibly in the future.

We can ask then why the general appearance of the heliocentric system made Copernicus and his followers believe that it was real—why its close coherence, its intellectual harmonies had such power to convince them of its reality. And to this we reply that the existence of a harmonious order is a denial of randomness, and order and randomness are mutually exclusive. Moreover, anything that is random is meaningless, while anything that is orderly is significant.[1]

To recognise the principle at work here, think of the difference between a tune and a noise; or, more generally, between a message and a noise. Communication theory defines a noise, in contrast to any distinctive series of signals, as random sequence, and it says that, being random, noise conveys no information—means nothing. This implies an important difference in the identifiability of an ordered sequence, as compared with a noise. Any single message is represented ideally by only one configuration of signals, while for a noise the very opposite holds. No significance must be attached to any particular configuration of signals that are a noise; we must indiscriminately identify any one configuration of a noise with any other configuration of it.

This is true of any aggregate deemed random: we must assume that the chance events which compose it could have as well happened otherwise. And, by contrast, once we have recognised an aggregate of events as orderly and meaningful, we may not believe that they might as well have happened differently. Such an aggregate is an identifiable thing, possessing reality in the sense I have defined it; namely, that it may yet manifest itself inexhaustibly in the future. To distinguish meaningful patterns from random aggregates is therefore to exercise our power for recognising reality.

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[1] I disregard here statistical laws, as they apply to another level of reality. (See my *Personal Knowledge*, p. 390.)

Our capacity for discerning meaningful aggregates, as distinct from chance aggregates, is an ultimate power of our personal judgment. It can be aided by explicit argument but never determined by it: our final decision will always remain tacit. Such a decision may be so obvious, that in it our tacit powers are used effortlessly and thus their use remains unnoticed; our eyes and ears continuously commit us to such effortless decisions. But other decisions of the same class may be hard and momentous. A jury may be presented with a pattern of circumstantial evidence pointing to the accused. It is always conceivable that this pattern may be due to chance; but how unlikely a chance should they admit to be possible? Or else, what degree of coincidence should be deemed quite unbelievable? The prisoner's life and the administration of justice will depend on the decision, and there is no rule by which it can be decided. This is precisely why it is left to the jury to decide it.

I have said that reality in nature is a thing that may yet manifest itself inexhaustibly, far beyond our present ken. Something must be added to this description, if the pursuit of natural science is to be justified. Consider that the Copernican revolution was but a continuation of a structuring that had its origins in antiquity. Copernicus deepened and beautifully clarified a coherence transmitted to him by Ptolemy. And this triumph pointed further beyond itself in the mind of Copernicus. In Kepler, passionately embracing the system of Copernicus, its image was to evoke anew the kind of creative hunger which Copernicus had satisfied by discovering it. And the presence of yet hidden truth worked its way further. To Newton, Kepler's three laws appeared to hang covertly together and he established this fact by his theory of gravitation, which derived all three laws jointly from the mechanics of Galileo. Nor was this the end, for a quarter of a millennium later, Einstein was to find unsatisfying the coexistence of the Newtonian system with the electromagnetic theory of light, and was to discover an even deeper coherence reconciling the two.

The continued pursuit of science is possible, because the structure of nature and man's capacity to grasp this structure, can be such as is exemplified by this sequence of discoveries covering two millennia. It does happen, that nature is capable of being comprehended in successive stages, each of which can be reached only by the highest powers of the human mind. Consequently, to discover a true coherence in nature is often not only to discern something which, by the mere fact of being real, necessarily points beyond itself, but to surmise that future discoveries may prove the reality of the thing to be far deeper than we can at present imagine.

It may seem strange that I insist on a belief in the reality of theoretical suppositions as the driving force to discovery. Such belief would seem a conservative assumption, rather than a source of innovation. The positivist view of science would indeed claim that the major discoveries of modern physics were based on a sceptical attitude towards the framework of hitherto accepted scientific theories. The discovery of relativity involved the abandonment of the current conceptions of space and time, and quantum mechanics achieved its breakthrough by discarding Bohr's planetary system of electrons circling the nucleus. Einstein himself acknowledged that Mach's positivist philosophy had inspired his work and Heisenberg's quantum mechanics was deliberately framed to reduce atomic theory to a functional relation of observable quantities.

These revolutionary heresies may seem to contradict my thesis, but I think they fall in line with it, once I make clearer the opposite extreme of creative procedure, based on a firm belief in the reality of the current framework of scientific theory. We may recognise the prototype of such a feat in the discovery of America by Columbus. He triumphed by taking literally, and as a guide to action, that the earth was round, which his contemporaries held vaguely and as a mere matter of speculation. The egg of Columbus is the proverbial symbol for such breath-taking originality guided by a crudely concrete imagination. I remember having this feeling when first hearing of Einstein's theory of Brownian motion. The idea that the meandering oscillations of small floating particles seen by a botanist under the microscope, should reveal the impact of molecules hitting the particles in tune with the highly speculative equations of the kinetic theory of gases, impressed me as grossly incongruous. I experienced the same shock of a fantastic idea, when I heard Elsasser suggesting (in 1925) that certain anomalies observed in the scattering of electrons by solids may be due to the optical interference of their de Broglie waves. We had all heard of these waves since 1923, yet were astounded by the fact that they could be taken literally as Elsasser did.[1]

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[1] This paper was delivered as a lecture at Duke University, Durham, N.C., in February 1964. James Franck lived at that time in Durham and we met to discuss my talk. Franck began very quietly, almost in a whisper, saying: 'You know, I am one of those literals.' He clearly was very pleased. During his great career, spent among conceptual revolutionaries like Planck, Einstein, Bohr, Heisenberg, Schrodinger, Born, he must have often doubted the quality of his own genius and he was glad to find its place acknowledged in

This should remind us that the first great move towards the discovery of quantum mechanics was de Broglie's idea of the wave nature of matter. This revolutionary idea and Schrodinger's development of it into wave mechanics, shows no trace of positivistic influences. Add to this, that Max Planck, the founder of quantum theory, actively opposed Mach's analysis of science and also dissented from Heisenberg's claim of basing physical theories on directly observable quantities[1]; and that Einstein himself, whose principle of relativity served as an inspiration to modern positivism, was sharply critical of Mach's analysis of science as a mere relation of observed facts.[2] It appears then, that the predominant principle that shaped modern physical theory was not the positivist programme, but the transition from a mechanical conception of reality to a mathematical conception of it, which sometimes coincided with the positivistic programme for the purification of science.

We can thus bring the revolution of the twentieth century in line with the Copernican revolution of the sixteenth and seventeenth centuries. Both revolutions consisted in a stepwise deepening of coherence with a simultaneous extension of its range. The modern revolution differed from its precursor only in establishing mathematical harmonies in place of beautiful mechanical systems.

The mathematical image of reality is more abstract than the mechanical, but its capacity to point beyond its immediate predicative content is similar to that of the mechanical image. I have said that the fact that the wave nature of particles postulated by de Broglie could be confirmed by

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my analysis. I think that among great discoveries the one most purely based on a literal acceptance of current ideas, was Laue's discovery of the diffraction of X rays; but Langmuir too triumphed by the powers of a literal imagination, and many of Rutherford's discoveries were based on daringly primitive conceptions.

[1] Max Planck, *Scientific Autobiography and Other Papers*, Philosophical Library, New York (1949), p. 129, Tr. Fr. Gaynor. (Original title of this essay was 'Der Causalbegriff in der Physic' first published in 1948.) 'It is absolutely false, although it is often asserted, that the world picture of physics contains, or may contain, directly observable magnitudes only. On the contrary, directly observable magnitudes are not found at all in the world picture. It contains symbols only.' In this essay (p. 139) he also objects to the elimination of seemingly unverifiable statements: 'I must take exception to the view (a very popular one these days, and certainly a very plausible one on the face of it) that a problem in physics merits examination only if it is established in advance that a definite answer to it can be obtained. If physicists had always been guided by this principle, the famous experiment of Michelson and Morley undertaken to measure the so-called absolute velocity of the earth, would never have taken place, and the theory of relativity might still be nonexistent.'

[2] In his Autobiographical Notes 1. c, p. 53, Einstein writes about his re-definition of reality as follows: 'The type of critical reasoning which was required for the discovery of this central point was decisively furthered, in my case, especially by the reading of David Hume's and Ernst Mach's philosophical writings.' But Einstein did not confirm Mach's teaching that the Newtonian doctrine of absolute rest is meaningless; Einstein proved, on the contrary, that Newton's doctrine, far from being meaningless, was false.

diffraction experiments, came as a fantastic surprise to physicists. The discovery of the positron occurred just as unexpectedly in confirmation of a prediction contained unnoticed in Dirac's quantum theory of the electron (1928).

In my account of the Copernican revolution and of the modern revolution in physics, I have mentioned only in passing the contributions made by new experimental observations. But the examples I gave were typical of the way at this time experiments often followed their theoretical anticipation, the connection being sometimes not recognised at first. Usually, theoretical speculation and experimental probing enter jointly into the quest towards an ever broader and deeper coherence.

This brings up the question, how the actual process of discovery is performed. Much has been written about this with which I disagree, but for the moment I can put my own views only quite summarily. To see a good problem is to see something hidden and yet accessible. This is done by integrating some raw experiences into clues pointing to a possible gap in our knowledge. To undertake a problem is to commit one-self to the belief that you can fill in this gap and make thereby a new contact with reality. Such a commitment must be passionate; a problem which does not worry us, that does not excite us, is not a problem; it does not exist. Evidence is cast up only by a surmise filled with its own particular hope and fervently bent on fulfilling this hope. Without such passionate commitment no supporting evidence will emerge, nor failure to find such evidence be felt; no conclusions will be drawn and tested—no quest will take place.

Thus the anticipatory powers that we have seen at work in historical perspective, arouse and guide individual creativity. These powers are ever at work in the scientist's mind, because he believes that science offers an aspect of reality and may therefore manifest its truth ever again by new surprises.

In this essay I have tried to define the mental powers by which coherence is discovered in nature. But the coherence achieved by the Copernican Revolution filled with dismay those brought up on the medieval order of the universe. The Earth's central position which had been the symbol of man's destiny as the only thinking morally responsible being, was lost. Gone was the divine perfection of an immutable firmament encircling the place where fallen man was ever to strive for salvation beyond this place. 'It is all in pieces, all coherence gone', wrote John Donne as early as 1611.

The destruction was deepened by the revival of atomism. Dante had said of Democritos that he 'abandoned the world to chance'. And Dante was right. The assumption that all things are ultimately controlled by the same laws of atomic interaction, reduces all forms of existence to mere collocations of ultimate particles. Such is the kind of universe that we have inherited from the Copernican Revolution. In it no essentially higher things exist, nor can intangible things be real. To understand the world then consists in representing throughout all that is of greater significance in terms of less meaningful elements and if possible, in terms of meaningless matter. Accepting such a conception of truth and reality, man is confused by his own lucidity and blighted by his self-doubt.

The anti-metaphysical critique of science marks the stage at which a false conception of truth and reality attacks science itself, from which it had first originated. If we can overcome these false ideals of knowledge in science, this might set an example for our whole outlook and, backed by the very prestige of science, might help to overcome scientism everywhere.

Once the recognition of anticipatory powers in science establishes a conception of reality transcending tangible things, we might be able generally to acknowledge higher entities, intangible and yet real—as real as matter and yet meaningful. We shall recognise thus a cosmic hierarchy in which man has once more his own place.[1]

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[1] The basic ideas of this paper were first stated in Chapter 1, 'Science and Reality' of my *Science, Faith and Society* (1946), and summarised in the introduction to the Phoenix Edition (1964) of that book. For the relation of Copernicus to Medieval thought I have benefited from Ernan McMullin 'Medieval and Modern Science: Continuity or Discontinuity', *International Philosophical Quarterly* (1965), 103. Mr Rom Harré (Oxford) showed me unpublished evidence of Melanchton's anti-realistic views on astronomy expressed in his violent attack on Averroes. Mr Harré, Professor Samuel Sambursky (Jerusalem) and Mr J. R. Ravetz (Leeds) all read my manuscript and suggested corrections from which I substantially benefited. Dr J. D. North (Oxford) helped me not only by commenting on the finished manuscript, but also before this, by discussions during my final formulation of my historical account of Copernicus. But I am, of course, responsible for all the content of this paper.

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Probability Kinematics*

by ISAAC LEVI

I

Richard Jeffrey's recent book[1] is a contribution of major importance to a unified theory of probability and utility. In addition, it is full of interesting suggestions regarding the way in which a Bayesian decision theory can be exploited to shed light on epistemological issues. Yet, in spite (or, perhaps, because) of the imaginative character of his suggestions, some of them are open to serious question.

In this paper, I intend to focus attention on Jeffrey's chapter on probability kinematics.[2] This chapter is devoted to considering how rational agents ought to revise probability judgments in reaction to their experiences. His remarks bear the imprint of the subjectivist approach to probability which he adopts throughout his book; but they involve no deep commitment to the specific variant of the subjectivist approach encountered on earlier pages of his book. Critical scrutiny of the contents of this chapter, therefore, presupposes some familiarity with subjectivist ideas but does not require intimacy with the details of Jeffrey's own approach.

Let Jones be a rational agent who at time t has at his disposal total evidence and background information b. Let $A_1, A_2, \ldots A_n$ be a set of propositions which are exclusive and exhaustive relative to b and each one consistent with b. (For the sake of simplicity only finite partitions will be considered.) Let M be the set of 2^n propositions obtained from the A_i's by forming the disjunctions of 1 or more of the A_i's and by adding the proposition F which is the contradictory of the proposition T, where T is the disjunction of all of the A_i's. T is entailed by the evidence b and F is inconsistent with b. Jones is taken to be concerned to assign probabilities to elements of the set M—perhaps, because these propositions are somehow relevant to a decision he is facing.

Assuming that Jones is not merely rational but ideally situated in the sense that his probability assignments to the elements of M are definite numerical values, he is obliged (in the interests of his rationality) to use a probability function defined over M which is coherent in the following sense:

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* Received 27.i.67

[1] R. C. Jeffrey, *The Logic of Decision*, New York: McGraw-Hill (1965).

[2] Ibid. ch. 11.

(*a*) The probability function 'prob' is non-negative.
(*b*) prob $(T) = 1$.
(*c*) If H and G are in M and H & G given the evidence is equivalent to F, then prob $(H \mathrm{v} G) =$ prob $(H)+$prob (G).

The coherence conditions (*a*), (*b*) and (*c*) are imposed on the assumption that no element of M is evidence or entailed by evidence other than T. However, because we shall want to consider how Jones's probability function over M ought to change when at some later time t' he acquires new evidence which is expressible by means of some element in M, condition (*b*) ought to be modified as follows:

(*b′*) If b & E is the total evidence where E is an element in M, then prob $(T \& E) = 1$.

All writers on subjective probability would require of a rational agent that his probability function over M be coherent in the sense just indicated. Some would require, in addition, that the function be coherent in a still stronger sense requiring fulfilment of the following condition:

(*d*) If G is an element of M which is not entailed by b & E, then prob $(G)<1$.

In the subsequent discussion, it shall be assumed that rational agents use coherent probability functions in making assignments to elements of a set of propositions M at a given time t.

Jeffrey's first question in the chapter on probability kinematics is how a rational agent ought to revise probability assignments when he acquires new evidence.[1] Assuming that the new evidence is expressed by a proposition E in M, coherence does require that he revise his probability function: for at t' the probability function PROB must be such that PROB $(E) = 1$. This implies revisions of other probability assignments to elements of M, but it does not necessitate that these assignments be made in a unique way. PROB could be any one of an infinite number of coherent functions. Thus, if coherence is the only obligation imposed on a rational agent in assigning probabilities to propositions, acquisition of new evidence dictates very little to him regarding how he is to revise his probabilities. New evidence is virtually irrelevant to the revision of probabilities.

If, therefore, evidence is to determine revision in probabilities, an additional requirement must be imposed on the rational agent. The one which is usually proposed is what Jeffrey calls 'conditionalisation'.

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[1] Loc. cit. especially pp. 155-6 where Jeffrey is clearly concerned with probability kinematics as a system of norms for probability revision and not as a descriptive theory of human behaviour.

Let $\text{prob}\ (H/E) = \dfrac{\text{prob}\ (H\ \&\ E)}{\text{prob}\ (E)}$ where $\text{prob}\ (E) \neq 0$—i.e. where E is not inconsistent with the evidence. Conditionalisation requires that if at t' Jones acquires new evidence expressed by the proposition E in M, the function PROB used at t' is related to prob (used at t) as follows:

$$\text{PROB}\ (H) = \text{prob}\ (H/E).$$[1]

If an agent follows the conditionalisation principle, then it would appear that his probability assignments are uniquely determined by his new evidence and the coherent probability function used prior to the acquisition of the new evidence. Jeffrey finds little to criticise in conditionalisation when the conditions for its application are present. However, there are situations, so he maintains, where an agent makes an observation but does not, as a result, accept any new evidence. For example, if someone draws a disc from an urn and observes it in poor light, he may not be able to discern its color. Yet, he might think it more likely to be blue than he did prior to observing the disc. The probability of a proposition (presumably in the appropriate M) has been altered in response to sensory stimulation, yet no new evidence has been obtained. Jeffrey holds it to be important to have a surrogate for conditionalisation which will cover situations such as this.[2]

The chief thesis to be propounded here is that Jeffrey's search for a substitute for conditionalisation applicable in cases where conditionalisation is inappropriate is fruitless. Arguments will be offered to support the view that when no new evidence is acquired, alterations in probability judgments are not in any relevant sense susceptible to justification. If this claim is correct, any proposed surrogate for conditionalisation will be arbitrary. The arbitrariness of Jeffrey's own proposal will be illustrated by applying it to an elementary statistical example. The illustration is intended to provide additional corroboration for the chief thesis. Finally, Jeffrey's attempt to exploit his proposals to escape commitment to incorrigible evidence sentences will be examined.

II

Suppose that at time t, Jones is confronted with an urn which he knows contains 10 discs. He also knows that the urn contains either 5 blue and 5 green discs or 10 blue and 0 green. Let the first hypothesis be H and the second $-H$. He also knows that one disc is to be drawn and that it will be either blue or green. Let E be the proposition that the disc is blue and $-E$ that it is green. M will consist of all disjunctions of 0 or more of the following three propositions: $H\&E$, $H\&-E$, and $-H\&E$, $T = H\mathrm{v}-H = E\mathrm{v}-E$ and $F = H\&-H = E\&-E$. Prob is Jones probability function over the elements of M at t.

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[1] R. C. Jeffrey, *The Logic of Decision*, pp. 153-4.

[2] Ibid. pp. 154-7.

At t', Jones has an opportunity to observe the disc drawn. There are three cases to consider:

(1) Jones somehow fails to observe the disc at t' but changes the probability of E from 0·6 to 0·9 (due, perhaps, to a change in his blood chemistry, to a newly acquired penchant for blue chips or what have you).

(2) Jones observes the disc and in response admits E (that the disc drawn is blue) into his evidence.

(3) Jones observes the disc. He admits no new proposition into his evidence but shifts the probability of E from 0·6 to 0·9.

Consider case (1) first. Is Jones irrational in his shift from prob to a new probability function prob′ at t'? The question as it stands is somewhat misleading; for it is concerned with the rationality of Jones rather than his probability judgments. It is the latter with which we are, strictly speaking, concerned. But when Jones's probability judgments are made the centre of attention, the question splits into two:

(*a*) Are Jones judgments at t' (and, for that matter, those at t) rational?

(*b*) Is the shift from the judgments made at t to those made at t' a rational one?

As long as the probability functions prob and prob′ used at t and t' respectively obey the requirements of coherence, subjectivists are inclined to give an affirmative answer to (*a*). The answer to the second question is not so easy. The relation between Jones's judgments at t and his judgment at t' resemble the relations between the judgments of Jones and Smith at t when they share the same evidence but use different coherent probability functions. Question (*b*) becomes something like asking is there anything irrational in the disagreement between Jones and Smith?

Presumably, we could say in the Jones-Smith case that the disagreement was arational in the sense that no reasons could be adduced to decide one way or the other. Agreement is not to be won by showing that the evidence justifies one system of probability judgments rather than another. Other methods of persuasion might do, but not an appeal to reason.[1] To be sure, a subjectivist could say that it is desirable that Jones and Smith agree in their probability judgments when they agree on the evidence; but the subjectivist cannot say that when this desirable state of affairs is not realised the opinions of either Jones or Smith must be irrational. The desirability of avoiding arational disagreement (which might be canonised as a condition on the rationality of agreements and disagreements) cannot be converted into a condition on the rationality of either Jones's beliefs or Smith's.

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[1] That is to say, not by an appeal to reasons available to Jones and Smith at time t. Under certain conditions, reasoned agreement might be obtained if Jones and Smith acquire the right kind of evidence at some later time.

A similar observation is pertinent in the Jones-Jones case. The transition from prob to prob′ is arational in the sense that although it might be possible to adduce explanations for the shift, no reasons can be cited justifying the shift. Again, it would be desirable (perhaps) for Jones to revise his probability judgments only when he had good reasons for doing so. But this condition on rational *revision* of probability judgments cannot be converted into a condition on rational probability judgment *at a given time* (or time interval). To do so would be to imply that Jones was irrational in using prob′ at t' simply because he happened to be using prob at t. But since, from a subjectivist point of view, Jones's using prob′ at t would have been just as rational as his using prob at t', it must also be the case that his using prob′ at t' is as rational as his using prob at t'.

Advocates of logical interpretations of probability such as Keynes and Carnap had hoped to convert conditions on rational revision of probability judgments and rational agreement in probability judgments into conditions on rational probability judgment at a given time. That is to say, they wished to add to the coherence requirements conditions sufficiently strong to insure that the probability judgments of a given agent at a given time be uniquely determined by the evidence available to him. Subjectivists doubt that plausible conditions strong enough to achieve this goal can be devised. Consequently, for them, situations where disputes can be adjudicated by appeal to reasons and changes in opinion can be justified by reasons are not the rule. Criteria governing rational adjudication of disputes and reasoned revision of judgments cannot enforce condemnation as irrational of opinions held by an agent at a given time—provided his opinions are coherent. The disputes in which he is engaged may be condemned as unamenable to rational settlement and his matter of changing his views may be deemed capricious—but not his opinions themselves.

These remarks regarding case (1) apply *mutatis mutandis* to case (2). In case (2), coherence will obligate Jones to revise his probability judgments due to his admission of E into his evidence. However, there are an infinite number of probability functions which will fit the requirements. No matter which of these coherent functions (let the one chosen be prib) is adopted, the judgments based on it at t' will be rational, from the subjectivist point of view. However, one can still ask whether the shift from prob at t to prib at t' is justified by an appeal to reasons. To be sure, the admission of E into the evidence requires a shift from prob to some other probability function. Can the admission of E into evidence justify transition to the particular function prib?

The usual answer would be affirmative provided that for any element H in M, prib $(H) =$ prob (H/E) as conditionalisation requires. From a subjectivist viewpoint, therefore, conditionalisation is a generalisation of the requirement imposed in case (1) that a reasoned shift in probability judgments requires a shift in evidence. It indicates how, when there is a shift in evidence, a reasoned shift ought to go. But, to repeat what should now be clear, the judgments of an agent who forms them in violation of conditionalisation are not, therefore, irrational. The change in judgment is not justified. Failure to justify revisions in one's beliefs may be a sin against reason; but it is a sin against canons for (rationally) revising beliefs and not against standards for determining the rationality of beliefs at a given time.

As is to be expected, advocates of logical interpretations of probability are unhappy with according this limited status to conditionalisation. They would like to say in case (2) that if the function prob used by Jones at time t met the conditions of rationality which they would like to impose, the function prib used by him at t′ must be obtained from prob via conditionalisation if the judgments based on prib are to be deemed rational. Conformity to the dictates of conditionalisation is not only a condition of reasoned revision of opinion but of rational opinion.

I have gone through this rather tedious discussion for the sake of case (3) which is precisely the kind of situation worrying Jeffrey.[1] Here, as in case (1), Jones revises his probability judgments (say from prob to prab) without having acquired any new evidence. In neither case, is the rationality of the judgments framed at either t or t' called into question. In case (1), the *shift* in probability judgments is without reason. *Prima facie*, the same is true in case (3). Yet, Jeffrey feels that case (3) is comparable to case (2) where the shift from prob to prib could be justified. The justification of the shift from prob to prab must, of course, be different from the justification of the shift from prob to prib. Yet, it must be justifiable in a similar sense.

Why does Jeffrey feel that case (3) ought to be treated like case (2) rather than case (1)? Presumably because in case (3) the shift in probability judgments is in response to an observation of the disc which is drawn just as in case (2). (Observe that the admission of E into evidence in case (2) is tantamount to shifting its probability from 0·6 at t to 1 at t'.) In case (1), on the other hand, the shift is in response to causes having nothing to do with the colour of the chip drawn or with any observation of that chip.

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[1] R. C. Jeffrey, *The Logic of Decision*, p. 154. My example is different from the one presented by Jeffrey. But the problem illustrated is the same.

One might be tempted to suggest that perhaps case (3) is not completely described. Even though no element in M is admitted into evidence, the proposition O asserting that the chip drawn appears to be blue is. We could expand the set M so as to include O and extend the probability functions at t and t' accordingly. In this way, conditionalisation would become operative and case (3) would be a variant of case (2).

But Jeffrey will have none of this. He wishes to consider cases (and they are surely possible) in which Jones fails to admit O or anything like it into his evidence.[1] If, however, he is serious about this, then in what relevant way is case (3) different from case (1)? Jones is in no position to justify his shift from prob to prob′ in case (1). Is he in any better position in case (3)?

Jeffrey's own way out of this difficulty seems to rest on the distinction between reasons and causes. In both case (1) and case (3), no reason can be cited justifying the shift in probability judgments. However, in both cases, causes can, at least in principle, be identified; and the difference between case (1) and case (3) is to be found in the difference in the causes of the shift.

> . . . Nor is it always a defect when beliefs are caused but not reasoned: when an observer justifies his belief that the sun is shining by saying, 'I see it', he is citing the cause of his belief. He is not blind; he is outdoors, with his eyes open; and in the circumstances he has no choice but to believe that the sun is shining, for he sees that it is. . . . . If the observer wishes to convince a doubter that the sun is indeed shining, he can do no better than to get the doubter to come outdoors and see for himself: to put himself into a position where his degree of belief in B will be forced to 1 by his sense experience. The doubter might also be convinced or nearly convinced by reasons, for example, by the consideration that it is noon in Fresno, California, on July 15, and that there, the sun is nearly always out then; but in such cases reasons are inferior to the right sorts of causes, as the proper sources of beliefs.[2]

Apparently, according to Jeffrey, causes can justify beliefs without being reasons for them. In case (1), the cause of the belief is not of the 'right sort'; but in case (3), it is. That is to say, in case (3) the shift in the probability of E from 0·6 to 0·9 is generated by a legitimising cause—to wit, the observation of the disc.

Let us waive any difficulties we might find in the distinction between legitimising and nonlegitimising causes. Observe, however, that the agent who appeals to the fact that he has observed the sun as justifying his belief that it is shining, presupposes that his witnessing the sun's shining under appropriate conditions is a cause of his belief and a legitimising one at that. If the person saw the sun shining but could not claim that this was a cause of his belief (or, in any event, that his observation reinforced a belief already present), he would not have, even on Jeffrey's own terms, justification for his belief.

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[1] Loc. cit.

[2] R. C. Jeffrey, *The Logic of Decision*, pp. 171–2.

Consider now case (3). Jones observes the disc and shifts the probability that E from 0·6 to 0·9. If he wishes to justify the shift, he might claim to have observed the disc in poor light and, in response, made the shift. Call the proposition expressed by this claim W. W is part of Jones's evidence bearing probability 1 at time t'. If it is not, then how could Jones justify the shift by citing the legitimising cause?

Now if the shift from prob to prab is a rational one, presumably it is because conditionalisation is applied—i.e. because prab $(E) = \text{prob}\,(E/W) = 0{\cdot}9$. In short, Jones could not cite causes legitimising his shift in belief without citing a reason justifying that shift. If Jones cannot cite reasons justifying the shift in belief, he cannot justify his shift at all. Either case (3) is like case (1) where justification is not possible or it is like case (2) where conditionalisation is operative. Jeffrey still has not shown why we need a surrogate for conditionalisation.

To these remarks, Jeffrey might reply that when new evidence is acquired as in case (2), no reason can be offered justifying the admission of that proposition E into evidence. Admission of E into evidence can be cited as reason for altering other probability assignments; but no reason justifies admitting E into evidence. By the same token, even though no reason can be offered for shifting the probability of E from 0·6 to 0·9 in case (3), that shift can be cited as reason justifying certain shifts in other probability judgments.

The analogy, however, breaks down at one critical point. In both case (2) and case (3), all probability assignments (or almost all) will be revised at t'. Some will involve increasing and other decreasing probabilities. How is Jones to distinguish those initial shifts for which he has no justification from those shifts which he can justify via an appeal to the initial shifts? Keep in mind that an appeal to legitimising causes will not do for reasons already cited.

In case (2), the answer is clear. Propositions bearing probability 1 on the new ordering are evidence. In case (3), no analogous criterion is available. No shift to some nonextreme probability value can be marked off as initial by appeal to the value reached. Jeffrey lacks a way of identifying initial shifts to be used to justify other shifts.

These remarks, if sound, suggest that Jeffrey's quest for a surrogate for conditionalisation is misguided. When conditionalisation is understood as a criterion for evaluating rational shifts in probability judgments (and, this seems to be the only available normative interpretation from a subjectivist point of view), situations where conditionalisation is inapplicable cannot be distinguished into cases where certain shifts can be justified relative to prior probability judgments and those where no shift can be justified. Case (1) and case (3) are indistinguishable with respect to justifiability. Of course, someone might suggest that a surrogate criterion of justification is required for both types of cases. Jeffrey does not appear, as far as I can tell, to think so; but if he did, we would be entitled to wonder what probability kinematics was supposed to be about.

III

Given that Jeffrey's search for a substitute for conditionalisation is a quest for a criterion for rational revisions in probability judgments where none is needed, his proposed criterion must be arbitrary. This implication can, I think, be confirmed by showing some cases where investigators would procede in direct violation of his instructions.

Jeffrey's 'surrogate' can be formulated as follows[1]:
Let $B_1, B_2, \ldots, B_n$ be elements in M whose probabilities shift from prob at t to PROB at t' in response to observations. Let $C_1, C_2, \ldots, C_k$ be all the consistent elements in M of the form $\pm B_1$ & $\pm B_2 \ldots \pm B_n$. Let G be an element in M.

$$\mathrm{PROB}(G) = \sum_{j=1}^{k} \mathrm{prob}(G/C_j)\mathrm{PROB}(Cj)$$

One difficulty immediately leaps to the eye. The shift from prob to PROB is generated by shifts in probabilities assigned to the B_i's not the C_j's. Jeffrey provides no method for determining the values of PROB (C_j) given the values of PROB (B_i) for each i and j. This is harmless in the case of Jones; for there E and $-E$ are both the B_i's and the C_i's. But this would not always be the case.

Ignoring this difficulty, however, another comes into view. Consider Jones and Smith at time t. Jones has not had an opportunity to observe the disc at that time, but Smith has. Smith, however, has not admitted any item into evidence not admitted by Jones. In this kind of situation, there would seem to be one respect in which Jones's and Smith's probability functions prob and prib ought to agree—to wit, prob (E/H) ought to equal prib (E/H) $= 0{\cdot}5$. When two agents are confronted with an urn containing blue and green discs from which one is drawn, and no other information is available, the probability of that disc's being blue given that the proportion of discs in the urn is p ought surely to be p. This point of view is one on which writers who admit statistical probabilities and subjectivists who, like De Finetti, wish to reject them would agree.[1]

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[1] R. C. Jeffrey, *The Logic of Decision*, pp. 161-3.

Now what is appropriate in the Jones-Smith case ought surely to obtain in the Jones-Jones case. Having admitted no new evidence at time t', Jones at that time ought surely to consider the draw from the urn to be a 'random' one. prib (E/H) ought surely to equal prob (E/H). Yet, Jeffrey's procedure would require otherwise. If prob (E) does not equal prib (E) but prob (H/E) = prib (H/E), as it does according to Jeffrey's principle, coherence dictates that prib (E/H) cannot equal prob (E/H).[2]

I do not wish to use this argument as a case for an alternative to Jeffrey's principle. It so happens that in certain kinds of statistical examples, Jeffrey's principle seems to conflict with well established practice. A statistician who took a fleeting glance at a sample from some population would rarely revise the likelihood function—even when interpreted subjectively—unless he admitted into his evidence some new proposition about the composition of the sample. However, not all cases where Jeffrey's theory is supposed to operate are statistical ones; and no intuitions are available to guide us for the nonstatistical problem. The point, therefore, is not that there is a better rule than Jeffrey's; but that any rule for case (3) is an arbitrary one. The arbitrariness is not surprising; for Jeffrey is attempting to impose conditions of rationality in a situation where none is called for.

IV

These difficulties with Jeffrey's account of probability kinematics constitute but a minor blemish on a masterly contribution to probability theory and the logic of decision. Nonetheless, the blemish is worth exhibiting; for behind Jeffrey's discussion lurks an epistemological question of considerable importance.

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[1] According to De Finetti, Smith and Jones would agree that an n-fold sample (with replacement) from the urn is an exchangeable series of events. Jeffrey's proposal would preclude this agreement when the circumstances are those described here.

[2] An interesting modification of the example results when $-H$ is the hypothesis that 7 of the discs are blue and 3 are green. Should E (that the disc drawn is blue) go to 0·9 upon quick observation in poor light, it would seem that prib (E/H) and prib $(E/-H)$ could not equal prob (E/H) and prob $(E/-H)$ respectively. This impossibility would not stem from Jeffrey's requirements but the requirements of coherence. It seems to me that in such a case, prib (E) would be revised downward in order to save the constancy of the likelihood function. In any event, a man who proceeded in this way could hardly be accused of irrationality in any plausible sense I can think of. Yet, Jeffrey would have to judge him irrational with respect to the way in which he alters probabilities.

'If anything is probable, then something must be certain.' So wrote C. I. Lewis.[1] According to the preceding discussion, this claim can be rephrased to assert that if shifts in probability judgments are to be supported by reasons or if disagreements over probabilities are to be adjudicable by an appeal to reasons, something must be certain. When so understood, Lewis's claim seems to be substantially correct—provided that to say that E is certain is to say that relative to the total evidence the probability that E is 1. In other words, if it is certain that E, E is deducible from the evidence and, in that sense, is part of it.

From this correct assumption, Lewis concludes that if something is to pass as empirical evidence, some empirical judgments must be incorrigible. This leads him to his account of expressive language and reports made in such language which allegedly have the right credentials for incorrigibility. Jeffrey, quite rightly in my view, wishes to reject incorrigible observation reports of any sort. Hence, he wishes to provide an account of reasoned shifts in probability which avoids commitment to incorrigible empirical judgments. Now Jeffrey assumes, along with Lewis, that assigning probability 1 to propositions—i.e. admitting them as evidence—is committing oneself to their incorrigibility. Consequently, he wishes to provide an account of probability kinematics in which all empirical judgments receive probability less than 1 and where no empirical judgment is admitted into evidence.[2]

Jeffrey does not rest his case for the need to supplement conditionalisation with another procedure solely on this epistemological issue. As we have seen, he thinks such a procedure is required to handle cases where observations fail to yield new evidence regardless of whether observations sometimes result in the acquisition of new evidence or not. The argument in the previous sections was intended to show that when revision of probability judgments is a result of sensory stimulation without new evidence acquisition, the shift is irrational in the sense that no reason can be offered to justify the shift. However, if Jeffrey and Lewis are correct in supposing that admitting E as evidence is tantamount to a commitment to E's incorrigibility, the previous argument also shows that *no* change in probability judgments can be justified if a fallibilistic view of empirical knowledge is to be upheld.

The obvious way to avoid this predicament is to reject the assumption made both by Jeffrey and by Lewis that what is certain (bears probability

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[1] C. I. Lewis, *An Analysis of Knowledge and Valuation*, quoted by Jeffrey, op. cit. p. 156.
[2] Jeffrey, op. cit. p. 156.

1 or is accepted as evidence) is incorrigible. This suggestion is not a new one. Consider the following well known and justly celebrated passage from Peirce:

> That the settlement of opinion is the sole end of inquiry is a very important proposition. It sweeps away, at once, various vague and erroneous conceptions of proof. A few of these may be noticed here.
>
> 1. Some philosophers have imagined that to start an inquiry it was only necessary to utter a question whether orally or by setting it down upon paper, and have even recommended us to begin our studies with questioning everything! But the mere putting of a proposition into the interrogative form does not stimulate the mind to any struggle after belief. There must be a real and living doubt, and without this all discussion is idle.
>
> 2. It is a very common idea that demonstration must rest on some ultimate and absolutely indubitable propositions. These, according to one school, are first principles of a general nature; according to another, are first sensations. But in point of fact, an inquiry, to have that completely satisfactory result called demonstration, has only to start with propositions perfectly free from all actual doubt. If the premises are not in fact doubted at all, they cannot be more satisfactory than they are.
>
> 3. Some people seem to love to argue a point after all the world is fully convinced of it. But no further advance can be made. When doubt ceases, mental action on the subject comes to an end; and, if it did go on, it would be without purpose.[1]

What counts as evidence, according to Peirce, are propositions which, in the context of inquiry at a given time, are not subject to actual serious doubt. It is pointless, therefore, to question evidence, to look for further evidence in support of evidence, to conduct tests of the evidence, insofar as it is taken as evidence.

Furthermore, what is not subject to serious doubt at a given time might become so at some other time. When this happens, the erstwhile evidence can no longer be considered as evidence. Evidence, in short, in not incorrigible. Assignment of probability 1 to a proposition is not a commitment to its infallibility.

Rejecting the Lewis-Jeffrey assumption does, of course, raise new problems. Sometimes propositions are admitted into evidence without justification—e.g. in reaction to sensory stimulation, due to the influence of tradition or the dictates of authority. In such cases, no account of reasoned admission into evidence is needed; for as long as the testimony of the senses and the dictates of tradition and authority are not called into serious question, no justification is demanded and none is needed.

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[1] C. S. Peirce, 'The Fixation of Belief', in *The Philosophy of Peirce*, edited by J. Buchler, Harcourt Brace and Company (1950), p. 11.

But what Jones takes as evidence might on some occasions be seriously questioned by him or by others. Then the erstwhile evidence must be removed from that status and some effort made to decide in the light of other evidence whether it deserves readmission. Criteria for rational admission into evidence are needed to cover such important situations.

This is not the appropriate place to consider such criteria in detail.[1] However, it is important to keep in mind that there is an alternative to Jeffrey's approach to the question of the incorrigibility of evidence. Lewis is, according to this approach, correct in supposing that probability presupposes certainty. He and Jeffrey are wrong, however, in maintaining that certainty presupposes incorrigibility.

Indeed, Jeffrey's view is a betrayal of fallibilism—not a defence of it. To the extent that our epistemic commitments are reflected solely in non-extreme probability judgments, we do not risk error. If Jones assigns a probability of 0·999 to *E*, then no matter what truth value *E* has, Jones's 'belief' (if one can call it that) is not false. Hence, if all and only indubitable propositions are to be accorded probability 1 and only such propositions are to be believed (accepted as true), the result is not fallibilism but a severe skepticism based on rationalist standards of epistemic worth. No proposition which might be false can justifiably be believed. No legitimate belief is subject to error.

In contrast, the alternative suggested here allows that propositions accorded probability 1 are liable to be false. In addition, it implies that there are conditions when a hypothesis *H* which has been subject to serious doubt and critical scrutiny may be admitted into evidence with a justification based on other evidence *E* which does not entail it. The ramifications of this approach do admittedly stand in need of further examination. But the position is frankly fallibilistic. Empirical propositions can justifiably be believed and, indeed, admitted into evidence even though it is possible that they are false. Jeffrey's rejection of this view (and he is not alone in this although his position is by far the most sophisticated of its kind currently available) is an abandonment of fallibilistic empiricism. Taking note of this fact is not to refute Jeffrey. But serious fallibilists will think twice before accepting what he has to say.

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[1] Some elements of a theory of evidence are proposed in I. Levi, *Gambling with Truth: An Essay on Induction and the Aims of Science*, New York: Alfred E. Knopf (1967), ch. IX.

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Logic and Inexact Predicates*

by DHARMENDRA KUMAR

It is generally agreed that (*a*) predicates like 'red', 'chair', 'one meter long' whose application to an object depends on the object's similarity to certain objects regarded as standard examples, and also (*b*) predicates defined in terms of such predicates, are to some extent 'vague' or 'inexact'; that for every such predicate there are or could be objects to which the predicate could neither be said (with justification) to apply nor not to apply. The term 'empirical predicate' is generally so used that most, if not all, empirical predicates would be of this kind. Max Black thought that the laws of logic as usually formulated could not apply to these predicates, and suggested a reformulation. A decade later Professor Körner claimed that classes determined by such predicates admitted more than the usual five basic relations and that it was possible to construct 'a non-Boolean algebra which would adequately reflect the use of ostensive predicates' (*Mind*, 1951). In a paper in this *Journal*, 1963-4 and subsequently in his book *Experience and Theory* he claims to have given the logic underlying 'empirical discourse'. I propose to examine his claims and to show that he is mistaken on several points.[1]

Professor Körner specifies three possibilities for an individual in relation to an 'inexact class', a class determined by an 'inexact predicate'. It is either a member of it or a non-member (and thus a member of the complement) or 'a neutral candidate for membership'. A statement ascribing membership of an inexact class to an individual thus has three possible, let us say, truth-determinations; it is neither true nor false but neutral if the individual is a neutral candidate for membership. It may be pointed out that there are two alternative subjects for the predicate 'neutral' which may be applied either to statements (or propositions) or to individuals in characterising their relationship to a class, ***but not to both.*** In the latter case, 'propositions which state of individuals that they are neutral candidates for membership of an inexact class', like those ascribing membership or non-membership, are either true or false, not neutral. This is obscured

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[1] I am indebted to Professor R. B. Braithwaite for his valuable comments on an earlier draft which have helped to make my account clearer.

by Professor Körner's expressions like 'three kinds of unanalysed atomic propositions, namely: (i) p_0, i.e. p_0 is true, (ii) $7p_0$ i.e. p_0 is false, and (iii) $^*p_0$ i.e. p_0 is neutral' (E.T. p. 36) which seem rather to lump together primary and secondary propositions, atomic and truth-functional ones. Besides individuals it is propositions rather than statements to which he applies the predicate 'neutral'. But he observes that 'neutrality is not an independent third truth-value' (p. 36) and that 'even for an n-valued system of logic with $n > 3$ independent truth-values, the need might arise to admit neutrality of some propositions' (p. 37). It follows that 'the Lukasiewicz-Tarski type of three-valued logic, in which the third value cannot turn out to be (or be turned into) one of the other two' (p. 36) cannot 'do justice to neutral propositions'. He thinks, however, that Kleene's strong three-valued tables given below (with '1', '0' 'X' substituted for 't', 'f', 'u') provide 'a suitable logical system'.

p	q	$p \wedge q$	$p \vee q$	$p \rightarrow q$	$p \equiv q$
1	1	1	1	1	1
1	0	0	1	0	0
1	X	X	1	X	X
0	1	0	1	1	0
0	0	0	0	1	1
0	X	0	X	1	X
X	1	X	1	1	X
X	0	0	X	X	X
X	X	X	X	X	X

p	$7p$
1	0
0	1
X	X

A glance at the tables shows them to be *regular* extensions of the tables of the classical functions for which the symbols defined by Kleene are customarily used. They are regular extensions in the sense that in the additional or non-classical rows—in each of which at least one argument has 'X'—the function is assigned '1' (or '0') if and only if (*a*) one of the arguments in the row has a classical truth-determination *w* and (*b*) the function is assigned '1' (or '0') in *both* the classical rows in which the argument has *w*, and thus irrespectively of the value of the other argument. This is inevitable for tables that 'would do justice' to both neutral and non-neutral propositions. As Professor Körner recognises, 'neutrality is neutrality between the two truth-values' (p. 36) or impossibility of determining the truth-value. If, therefore, the truth-determination of a function supposed to be used in empirical discourse is invariant with respect to the truth-value of one of its arguments for some values of its other arguments, it should be unaffected by the impossibility of determining the ineffective truth-value of that argument. This is how Professor Körner's guiding principle that 'the rules governing their composition and relations must be so chosen that for propositions that are not neutral and classes that are exact, they reduce to the rules of classical elementary logic' (p. 36) must be interpreted if it is to make sense. For the *choice* of functions which 'reduce' to classical functions 'for propositions that are not neutral' is limited to a choice of their values in the non-classical rows, the classical rows of every non-classical function being necessarily identical with the table of some classical function. It is also necessary to assign 'X', as Kleene does, to every function in the last row (in which all arguments have 'X'), at least for the reason that it is possible for a neutral proposition to 'turn out' to be true or false and thus to affect the truth-determination of the propositions of which it is a component.

While the truth tables selected by Professor Körner thus 'do justice' to neutral empirical propositions, and the conditions he formulates for any logic that would serve the purpose may be said to be correct on a plausible interpretation, he seems to be unaware of their consequences. On the one hand he is satisfied that the tables conform to the fact that 'neutrality is not an independent third truth-value' (p. 36), and also says that they fulfil his hope of neutral propositions' 'plausible incorporation in a system of modal logic' (p. 37); on the other hand he seems to have been misled by Kleene's statement that his '3—valued logic . . . is not the same as the original 3—valued logic of Lukasiewicz' (*Introduction to Metamathematics*, p. 335) into supposing that the tables determine a propositional logic whose valid forms constitute a proper subclass of classical tautologies, different from any determined by a 'Lukasiewicz-Tarski type' of three-valued logic. Thus he says that 'not all valid (or identically true) *forms* of classical elementary logic remain valid forms in the [Kleene] three-valued logic', that '$7(p \wedge 7p)$ is not valid' (p. 41); and he describes what he claims to be a 'method . . . of defining and determining logical truth in the three-valued system' (p. 40). Unfortunately, he does not give us a *form* valid in this 'logic', and his method of determining whether a *proposition* is 'logically true' according to this 'logic' makes logical truth dependent on the *actual* truth-determination (truth value or neutrality) of its components rather than on its form, so that '$p_0 \vee 7p_0 \vee q_0$ is logically true' but '$q_0 \vee 7q_0 \vee p_0$ is not logically true' 'if p_0 is true, and therefore $7p_0$ false, and q_0 is neutral and therefore $7q_0$ is neutral' (p. 41).

Two alternative complete three-valued logics with the same designated value, as distinguished from alternative formulations differing in the choice of primitive functions, are no more possible than alternative complete two-valued logics. Professor Körner is wrong if he thinks, as he seems to in contrasting Kleene's 'logic' with 'classical logic', that the former is *complete* in the sense in which the latter is. It is possible, however, to have an incomplete or even a complete three-valued logic whose valid forms constitute a set different from that of the valid forms constructed by means of Lukasiewicz's implication and negation alone which, as is well known, cannot define all the functions of three truth values. But a three-valued logic, complete or incomplete, requires a set of functions capable of defining the 3-tautologous function, the tautologous function of arguments ranging over three values. A set of functions like Kleene's, of which none has a classical value for a set of non-classical argument-values, is incapable of defining the 3-tautologous function. (In fact, Kleene's functions cannot define any function having the same value for all sets of argument-values.) This can be easily shown by strong (or complete) induction on the number of occurrences of the connectives in the schemata. Every schema containing exactly one occurrence of any of the connectives defined by Kleene has 'X' in the last row. Supposing that every schema containing *less than m* occurrences of the connectives has 'X' in its last row, it is obvious that every schema containing *m* occurrences of the connectives will have 'X' in its last row, since it will be either the negation of a schema which has 'X' in that row, or a conjunction or disjunction or conditional or biconditional of components having 'X' in that row. It follows that no schema containing only Kleene's connectives can be 3-tautologous. And there is no other satisfactory criterion for marking out a subset of classical tautologies. If, for instance, 'valid forms' are identified with schemata having no '0' in the table, their set becomes identical with the set of classical valid forms; for it can be shown by induction that a schema which has '0' in a non-classical row also has '0' in at least two classical rows, the rows in which (*a*) one of the arguments which has 'X' in the non-classical row (in which the schema has '0') has '1' and '0' respectively and (*b*) the arguments which have '1' or '0' in the non-classical row have that very value; a schema which has no '0' in the classical rows cannot have a '0' in a non-classical row either. The identity of the columns of the two components can serve as a criterion for biconditional valid forms. On this criterion certain pairs of schemata containing different connectives like '$(p \rightarrow q) \rightarrow q$' and '$p \vee q$', or containing different letters like 'q' and '$q \wedge (r \vee \neg r)$', are not equivalent, and biconditionals formed from them not 'valid forms'. But for these differences the third value in Kleene's tables is a dummy. And there is no criterion applicable to schemata other than biconditionals—including criteria which ignore the last row—on which '$\neg(p \wedge \neg p)$ is not valid'.

Kleene's tables thus provide only a logic of secondary propositions, a systematisation of the inferential relations between propositions ascribing certain predicates to atomic propositions and propositions constructed from them by means of certain connectives. This logic is rather close to intuitionistic logic of secondary propositions ascribing provability, etc., to propositions in admitting that a disjunctive proposition is provable if and only if one of its disjuncts is provable; '$p \rightarrow (p \vee q)$' interpreted as 'if "p" is provable then "$p \vee q$" is provable' holds as the row in which 'p' has the value '1', '$p \vee q$' has the same value, while '$p \vee \neg p$' interpreted as ' "p" is provable or "$\neg p$" is provable' does not hold as there is a row in which neither 'p' nor '$\neg p$' has '1'. Kleene constructed these tables for propositions ascribing partial recursive predicates 'which may be undefined for some value of x'. As he observed, the law of excluded middle applies classically but not intuitionistically to these propositions; if any of these propositions is not decidable by algorithm, neither is *any* function of it. Professor Körner mistakes (*a*) the predicates which admit of no algorithm, discovered or undiscovered, for deciding the truth value of a proposition ascribing it to an object for (*b*) predicates 'for which so far no method is available by which to decide . . .' (p. 38); and his belief that neutrality ascribable to Kleene's functions and their arguments is 'not an independent third truth value' seems to be based on this mistake (together with Kleene's statement that it is simply the absence of information regarding the truth value) rather than on a consideration of the meaning of 'independent truth value' in terms of truth functions. For he says that 'the important similarity between' Kleene's and empirical neutral propositions consists in the fact that the former 'may sometimes' and the latter 'may always be turned into' true or false ones. Decidability as true and as false together with undecidability with respect to a given state of algorithmic development do satisfy Kleene's tables, however, since the undecidability of every function of argument undecidable relative to a set of operations or stage of knowledge is independent of whether or not the arguments are decidable by some other set of operations. The neutrality of an atomic empirical proposition which Körner characterises as 'neutrality between the two truth-values' consists in the impossibility of determining the truth-value either by a given set of operations or by any set of operations whatever. It is obvious that Kleene's tables are satisfied by the properties: (A) being decidable as true by a given set of observational operations or as a true function of arguments decidable as true or as false by the given set of observational operations, (B) being decidable as false similarly, and (C) being neither decidable as true nor decidable as false by these operations. Even the properties designated by the predicates obtained by substituting 'any observational operation' for 'a given set of observational operations' in the predicates used above satisfy the tables in the same strong sense of each property characterising some atomic proposition and some of its functions. No apparatus, however refined, can enable us to decide the truth value of every proposition ascribing a spatial distance in millimetres to any finite decimal place. But in common parlance it is rather in the sense of undecidability by some (or any) set of observational operations *and* the operations of classical logic that we speak of a proposition as neutral, undecidable or uncertain, and not in the sense of the property (C). And this neutrality together with the associated decidabilities do not satisfy Kleene's tables, since the tautologous functions of neutral arguments are decidable by classical logic. The decidability and undecidability of non-tautologous functions of empirical propostions does conform to these tables, since we speak of them as knowable or decidable if and only if their truth value follows from the truth values of their arguments by classical logic.

Professor Körner throughout uses the predicates 'true', 'false', and 'neutral' rather than 'can be known to be true . . .' etc. But he considers a 'modal logic' in which 'the modal operator N . . . [is] interpreted as "is true", the operator N7 as "is false", and the operator O as "is neutral" ', and the last one 'defined by $Op \underset{Df}{=} 7Np \wedge 7N7p$' (p. 37). It is not difficult then to see that 'N' is independent while 'O' is not, in the sense that 'O' is definable in terms of 'N' and truth-functional connectives while 'N' is not so definable in terms of 'O'. But he claims the neutrality of empirical propositions to be dependent in some *further* sense which a modal logic based on the above operators cannot bring out. He says that in such a modal logic 'we cannot choose to change Op_0 into Np_0 or $N7p_0$' and that 'a neutral proposition on the other hand *can* be turned by decision into a true or a false proposition' (p. 37). On the supposition that openness in such a modal logic may be interpreted as 'indifference' between being 'obligatory' and being 'forbidden' he argues that if an act is indifferent, 'performing it does not make it obligatory, and omitting it does not make it forbidden' (p. 37). If there is a morally indifferent act it can no more become obligatory by being performed than a forbidden one can. But why? Because the moral quality of an act is determined by the moral rules and not by one's performance or attitude. Whether an empirical proposition is neutral or not depends likewise on the rules of the use of the predicate occurring in it. It would seem then that one can no more terminate its neutrality than the moral indifference of an act by 'a free choice'. On account of his conviction of the provisionality of neutrality of empirical atomic propositions Professor Körner also denies its similarity to the undecidability of a proposition with respect to a theory, which like neutrality is determined by rules (and axioms). It is amazing that while rejecting the above-mentioned modal logic he could be so satisfied with Kleene's tables as to preface their first mention by 'fortunately;' for they constitute just one modal logic based on a set of three modal predicates. But while failing to notice the identity between his 'modal operators' and the symbols occurring in the cells of Kleene's tables, he does see the difference between (*a*) the undecidable propositions ascribing partial recursive predicates, for which *he thinks* 'a method might be found' (p. 38) for determining the truth value, and (*b*) neutral atomic empirical propositions which *he thinks* 'we can *always by a free choice*' (p. 38) turn into true or false ones. He contends, therefore, that Kleene's tables can 'only serve us in the provisional stage', and is led to his 'modified two valued logic'.

In this 'logic' Professor Körner distinguishes 'a provisional and a final stage of evaluation' (p. 43) of the truth value of a proposition and of the relation between two inexact classes. Every proposition is characterised by a provisional evaluation and a set of 'possible' final evaluations, and every pair of classes by a provisional and a set of 'possible' final relations, distinguished by the use of square brackets and braces respectively. Strangely enough, every provisional and every possible final evaluation is a classical evaluation, truth and falsehood for a proposition and one of the five fundamental relations for a pair of classes. The provisional evaluation of a proposition is the truth-value of the *residual* proposition that results if 'we ignore all neutral components together with their connectives' (p. 43), and seems to be denied to neutral atomic propositions; the provisional relation between two inexact classes is the relation between the corresponding classes obtained by ignoring neutral candidates for their membership. The possible final evaluations are the evaluations made after terminating the neutrality of neutral components of propositions by different possible choices of truth and falsehood, and of neutral candidates for membership of the classes by different possible choices of membership and non-membership. By banishing neutrality together with Kleene's tables, contrary to earlier statements, from the 'provisional evaluation' of propositions Körner makes the provisional evaluation pointless. To say that a proposition is neutral is hardly to give any information about its truth value, but the truth value of its non-neutral residue brings one no nearer the truth value of the whole proposition, as there is no relation of approximation between the truth value of a proposition and the truth value of a residue of it. On the contrary, it seems outrageous to characterise a tautologous proposition like '$(p_0 \wedge q_0) \rightarrow q_0$' as provisionally false as Professor

Körner would, given that 'p_0' is false and 'q_0' is neutral. The provisional relation between two empirical classes is the relation we usually have in mind, but in Körner's 'logic' it is redundant, as it is uniquely determined by the set of possible final relations. Thus if exclusion and overlap are the possible final relations, the provisional relation must be exclusion. His non-classical relations, inclusion-overlap and exclusion-overlap, defined by him in terms of provisional and possible final relations are definable entirely in terms of the possible final relations.

Professor Körner's use of '*t*' and '*f*' within braces to represent possible final evaluations is rather deceptive. To say of a given proposition that truth and falsehood are its possible final evaluations is hardly to say anything about its actual truth value. Assuming that neutrality is in some sense provisional, the only distinction worth making would be between the provisional neutrality and the final actual evaluation, and not between provisional neutrality and possible final truth values, much less between the truth value of a non-neutral residue and the possible final truth values of the whole proposition. The only way in which Körner can distinguish his '$\{t,f\}$' from Kleene's '*u*' is by using the former to indicate in addition that neutrality is convertible into either truth value by free choice. Similarly, by saying that inclusion and overlap are the possible final relations between two given classes he seems to mean that there is free choice between the two.

Professor Körner thinks that by characterising every empirical proposition and relation between every pair of empirical classes by a pair of classical evaluations, provisional and final, he can distinguish a logic from classical logic as the logic 'underlying' them, and yet admit that 'the valid forms of classical logic coincide with the valid forms of modified two-valued logic' (p. 45). Perhaps that is why he only speaks of relations 'in addition to . . . the classical relations' between classes, and not of a 'non-Boolean algebra' as he did in his article *Ostensive Predicates* (*Mind*, 1951). Even on the assumption that the forms of classical logic are not applicable to neutral propositions unless their neutrality is terminated, his ban on neutrality as a provisional evaluation is unwarranted, for only the logically possible final evaluations, whether or not the use of discretion or free choice is a part of their biography, are relevant to their inferential relations, and hence to their logical forms. From the supposition, therefore, that every empirical proposition has two evaluations, provisional and final, that some empirical propositions are provisionally neutral, and that neutral atomic empirical propositions can be made either true or false by free choice, it does not follow that 'the logic underlying empirical discourse' is different from classical logic. Classical logic is a logic defined by a set of valid forms or, alternatively, by the possible (final) evaluations of the propositions it applies to. The logic underlying empirical discourse cannot, therefore, be an alternative to or modification of classical logic, if every empirical proposition is finally either true or false. Körner's term 'modified two-valued logic' is, therefore, a misnomer. The word 'logic' is, however, used in other senses also, and one may speak of 'the logic of empirical propositions' in the sense of a correct elucidation of the concept of empirical proposition, in which provisional neutrality (in some sense of the expression) may find a place.

The above examination reduces Körner's 'modified two-valued logic' to the thesis that 'we can always by a free choice decide to turn a neutral proposition into a true or a false one'. In order to see whether educated human beings in fact treat neutral propositions as arbitrarily convertible into true or false ones and whether they can justifiably be so converted, it is important to see what exactly it is for an object to be a neutral candidate for the application of an ostensive predicate whose meaning is explained in terms of similarity and dissimilarity to certain objects regarded as standard examples and non-examples (examples of the contradictory or complement of the predicate). Firstly, unlike Max Black (*Language and Philosophy*, ch. 2.), Professor Körner seems to think that the neutral candidature of an object for the application of an ostensive predicate is not something to be inferred from an inconsistency of application of the predicate and its contradictory to the object by different persons. Instead he thinks it is as obvious to everybody as the applicability and non-applicability of the predicate to other objects. But while it is difficult to maintian that an object which is a neutral candidate for the application of a predicate is obviously so, it is more important to consider the neutrality which is independent of the intersubjective variation in the use of the predicate in ordinary discourse. The actual application of an ostensive predicate 'P' and its contradictory by a person depends on his stock of predicates contrary to 'P' and on the examples (of the predicates) he is acquainted with; and they vary from person to person. Thus a person acquainted with 'orange' besides 'red' and 'yellow' will apply 'non-red' to certain objects to which one not acquainted with 'orange' will apply 'red'. Similarly a person acquainted with objects drawn from only one end of the spectrum of yellow as examples of 'yellow' may apply 'yellow' and 'not-yellow' differently from a person acquainted with examples drawn only from the other end. Professor Körner does not seem fully to realise this. It is not enough to require, as he does, that the standard examples must not be 'borderline cases' (p. 25); for there is not just yellow and borderline yellow, but a whole spectrum of yellow. Such intersubjective variations as are only a feature of *ordinary* discourse are not essential to *empirical* discourse which includes the experimental or observational part of scientific investigation. Since Professor Körner is concerned to distinguish empirical discourse from *theoretical* discourse, and is not concerned particularly with ordinary discourse, it is important for him to consider neutrality which is independent of the latter.

Secondly, Professor Körner distinguishes 'neutral candidates' from 'undecidable candidates' which he attributes to 'incomplete qualifications and disqualifications' (p. 28) which 'one can supplement so as to make an undecidable candidate either a member or a non-member'. Unfortunately he gives no examples of such cases, and the predicates determining all three of his first examples of 'resemblance-classes . . . the class of green things, of elephants, of motor-cars' (p. 23) are perhaps so used as to leave room for supplementing the qualifications and disqualifications (henceforth QD) for their application and thus for making decidable some of the objects he says he finds or can imagine incapable of a decision. This is particularly obvious in the case of a complex predicate like 'motor-car'. The actual examples by which its meaning is explained at any given time do not allow a decision regarding its application to later creations—say, powered caravans—which are similar to the standard examples in some respects and dissimilar in others. But it is possible to supplement the QD and make these creations either members or non-members. This is what a decision by a law court or an academy does by making similarity or dissimilarity in a certain respect to the standard examples relevant or irrelevant to the application of the predicate. The verbal decision of the court can be substituted by addition of certain examples or non-examples. Unlike these commonly used complex and simple predicates which require a very large number of objects as examples, or at least a pair of meticulously selected examples and ordering of degrees of similarity, the highly determinate ostensive predicates employed by scientists—say, analytical chemists using colorimetric techniques—can be easily defined completely by a single example, or by just a few. It is obvious, if these predicates are considered, that neutral candidates as distinguished from undecidable candidates constitute the gap narrowed down to the limits of human sensory discrimination (with or without the use of instruments), however that may be distinguished from the varying sensory capacities of individual human beings which are not relevant to the question of QD for the application of a predicate belonging to a public language. Professor Körner's remark among others, that some people apply 'turquoise' to what seem to them borderline cases 'whereas others with sharper powers of discrimination' use it as an 'independent' predicate, suggests that he agrees in viewing neutral candidates as bound up with the limits of sensory discrimination. Parity between 'neutral candidates' assumed by him for all 'inexact-predicates' would again seem to be true of these predicates only.

The practical consequences of treating a neutral proposition as true are often not very different from those of treating it as false. Thus it is of no consequence whether the lump of sugar on the scales is regarded as weighing over 100 gm or not, so long as it is over 90 gm. in any case. But not to bother in such a situation whether the lump weighs over 100 gm. or not is to be content with approximation rather than to convert a neutral proposition into a true or a false one. Occasionally the practical consequences of regarding the proposition as true are different from those of regarding it as false, and then it is obvious that the decision is guided by the likely consequences rather than arbitrarily. If a court *has* to decide a neutral case, it decides by some principle like 'benefit of doubt to the citizen', and not arbitrarily by the whim of the moment or by the toss of the coin. And this amounts to restricting the application of the law to cases to which the predicate in question positively applies. In scientific investigation, and even in practical situations which are important enough to make use of scientific instruments, there is no room for the semblance of free choice. A scientist testing an hypothesis does not depend on the neutrality of the colour of his solution, but nor does the pilot, much less the missile commander on the neutral readings of his instruments.

Professor Körner seems to think that his thesis of free choice follows from the fact that a neutral candidate violates neither the qualifications nor the disqualifications for the application of the predicate. But if an object not qualified for the application of a predicate is yet not disqualified for its application, then how can the QD be said to be *complete*? If they are incomplete then free choice, which is confined to the rules for the use of an expression, is available, though not to each user of the expression as Körner suggests by speaking of 'the freedom of making up one's mind about neutral candidates' (p. 29); for its exercise on neutral candidates consistently with their similarities (not at random) has the effect of supplementing the QD. Professor Körner tries to reconcile the completeness of QD with the freedom of choice for neutral candidates by saying that 'in a similar manner the constitution of a club which gives some discretion to its appointment committee, is not on that account incomplete' (p. 28). But in order to leave room for discretion the QD (minus discretion) must be incomplete. He may say that this is compatible with his view that QD are complete only in the sense that they cannot be supplemented. But from the fact that QD cannot be supplemented, it does not follow that there is free choice in applying or not applying to neutral candidates.

The occurrence of neutral candidates is compatible with the completeness of QD, because neutrality arises not from incompleteness of QD but from the impossibility of determining for every object whether it satisfies the qualifications. This determination depends on operations of sensory discriminations which for many predicates have limitations. The set of neutral candidates for the application of a given ostensive predicate is relative to a set of operations for determining whether an object is qualified; what is a neutral candidate with respect to one set of operations may not be a neutral candidate with respect to another set of operations. But what can be known to be true by one set of operations cannot become known to be false by another set of operations, or vice versa. For most predicates having neutral candidates with respect to available operations it is not known that more sensitive operations are not discoverable. The very possibility of a conflict between (*a*) the results of arbitrarily converting neutral candidates (relative to a set of operations) for the application of any predicates at all into positive or into negative candidates and (*b*) the results of hitherto undiscovered operations shows that Körner's thesis of free choice is basically mistaken. If this thesis were true, there would be no point in seeking nor possibility of finding new and more sensitive operations and advancing knowledge.

A neutral proposition is thus not one which is neither true nor false, but one which cannot be decided as true or as false by a given set of operations. 'Neutral' thus belongs to a different category from that to which 'true' and 'false' belong. Classical logic applies to propositions which are neutral with respect to a given set of operations as much as it does to propositions which can be known to be true or to be false by this set of operations. According to classical logic, every proposition is either true or false, but not knowable as true or as false by a given operation or even by any of the discoverable operations; otherwise nothing could remain unknowable. Professor Körner is mistaken in supposing that if there were no free choice in dealing with neutral propositions, then what he believes to be Kleene's three-valued propositional logic, and not classical logic, would be applicable to empirical propositions. But, of course, classical logic cannot do the miracle of yielding conclusions that would be known to be true if the essential premises are neutral. Neutrality, like contingency, is carried over from premises to conclusion, and from atomic components to some non-tautologous truth functions in accordance with Kleene's tables.

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The Implicit Definition of Theoretical Terms*

by JOHN A. WINNIE

I

It is nowadays almost a truism that a theory which merits the title 'scientific' must be either confirmable or falsifiable by reports of possible observations. Above all, it is the theory's observable consequences that either justify its acceptance or reveal its inadequacies. Yet those scientific theories which have best demonstrated their fruitfulness are also those containing terms (which I shall call 'theoretical terms') which purportedly designate entities which are not observable, and, in some cases it seems, unobservable in principle. Here we have the basis of an epistemological problem which has been of perennial concern to philosophers of science: how do these terms which designate unobservables come to do so?[1] One can convey to others the sense of 'green' or 'cold' by, roughly, putting them in a position to see a patch of grass or touch a piece of ice. Such or similar methods are not available, however, for teaching the sense of terms like 'electron' or 'neutron'. To be sure, the presence of electrons can be indicated by a variety of observable happenings, such as white patches on the screen of a cathode ray tube; nevertheless, this is quite a different matter from observing an electron. But, even granting this, some philosophers believe that the above example does furnish a clue as to how we do come to understand what theoretical terms designate. For as one gradually learns which situations indicate the presence of, say electrons, the term 'electron' comes to be more and more meaningful; we learn that 'electron' designates a class of physical entities which, under certain observable conditions, illuminate screens of cathode ray tubes, produce white traces in cloud chambers, etc.; we learn that 'electron' designates *the* things that behave in just these particular ways under such-and-such conditions. So although we still do not have knowledge of the designation of 'electron' which is as direct and complete as our knowledge of the designations of 'green' or 'cold', yet we have an indirect and partial interpretation for the term which at least informs us of *some* of the characteristics of its designata.

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[1] There are, of course, some philosophers who deny that theoretical terms *do* designate theoretical entities, or anything at all. But my purpose here is to examine problems posed for the realist, not to adjudicate the realism-instrumentalism issue.

It follows, moreover, from this account that theoretical terms have no significance apart from the theory in which they are imbedded. For it is the *theory*, after all, that informs us that it is *electrons* (not tiny pebbles) that reveal their presence in cathode ray tubes and cloud chambers. It is the lawlike connections which the theory asserts to hold between electrons, other theoretical entities, and observables which provides the basis for the partial interpretation of the term 'electron' (and the theory's other theoretical terms as well). This is sometimes put succinctly by saying that the theoretical terms are *implicitly* defined by the nomological network (the system of lawlike statements) in which they are imbedded. The theoretical terms acquire their empirical content by 'upward seepage' or 'osmosis' (Herbert Feigl's phrases) from the observation terms (cf. [3], pp. 34-36; [5], p. 87; and [6], pp. 46-52). Before examining this view in more detail, let us state matters somewhat more precisely.

Following Carnap [1], let the primitive terms of the theory be divided into two classes: the theoretical terms and the observation terms.[1] Let the language in which the theory is formulated be first-order, and the theory be given by a finite set of axioms. The interpretation of the primitive observation terms is assumed completely specified, perhaps given ostensively, or in some other fairly 'direct' way. Now the axioms of the theory can be said to preclude certain interpretations for the theoretical terms, namely those which make the theory false. Thus, the axioms restrict the set of all possible interpretations of theoretical terms to a much narrower set—the set of true interpretations—and this, I think, is a fairly accurate explication of what is sometimes meant by saying that the axioms 'implicitly' define these terms (certainly by Nagel [5], p. 87 and Pap [6], pp. 51-52).

Were it not for the assumption that the observation terms have a fixed empirical interpretation, this would be a patently inadequate account of the empirical significance of theoretical terms. For since we have assumed our theory couched in the language of quantification theory, Lowenheim's theorem assures us of the existence of a true *arithmetical* interpretation of the theory if the theory be satisfiable at all. Once we assume a fixed empirical interpretation for the observation predicates, however, Lowenheim's result no longer applies. But, even though Lowenheim's theorem cannot be used in this instance, can we arrive at numerical interpretations of theoretical terms via another route? The following considerations would seem to indicate a negative answer to this question. First, any adequate reconstruction of a physical theory will contain sentences (called by Carnap and others, 'correspondence postulates') which contain both theoretical and observation terms. Any admissible interpretation of the theory's theoretical terms, then, must be such as to make these sentences come out true while retaining a fixed interpretation for the observation terms. But then the correspondence rules would truly assert connections between classes of theoretical entities and classes of observational entities—seemingly an impossible situation if theoretical entities are taken to be numbers. Furthermore, the theory may contain relation symbols which, under their intended interpretations, again relate theoretical and observational entities. So we would seem to have some assurance that true numerical interpretations for theoretical terms are not generally forthcoming. The theory's correspondence rules, in particular, would seem to weave an empirical web about the theoretical terms, guaranteeing that they designate classes of physical entities—not numbers. But if the result to be presented here is correct, then, under certain trivial assumptions, a true numerical interpretation for the theoretical terms will always be forthcoming (if there exists a true physical interpretation). Furthermore, the interpretation of the observation terms is not affected as the theoretical terms are now construed as designating classes of numbers. In short, the fact that an empirical interpretation of the observation terms is taken as fixed, does not rule out true numerical interpretations for the theoretical terms. As far as the doctrine of implicit definition goes, theoretical entities might just as well be construed as numbers. In the next section, this and a related result will be stated with proofs sketched.

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[1] Later, for the sake of greater generality, three classes of predicates will be distinguished.

II

Let the language $\mathscr{L}$ in which our theory $\mathscr{T}$ is formulated be first-order and contain, for simplicity's sake, a finite number of predicates as the only descriptive signs. These predicates are divided into three classes: $\mathrm{P}_0 = \{O_1, O_2, \ldots O_j\}$, $\mathrm{P}_t = \{T_1, T_2, \ldots T_k\}$, and $\mathrm{P}_m = \{M_1, M_2, \ldots M_l\}$. As will become clear later, these are the observation terms, theoretical terms, and 'mixed' terms, respectively. Rather than formulating $\mathscr{T}$ in a two-sorted language, we shall also introduce the one-place predicates '*Th*' and '*Ob*' which, intuitively, are read as 'is a theoretical entity' and 'is an observational entity', respectively. A *possible model* for $\mathscr{L}$ will then by any sequence $\langle \mathrm{Th} \cup \mathrm{Ob}, \mathrm{Th}, \mathrm{Ob}, \mathrm{O}_1, \ldots, \mathrm{O}_j; \mathrm{T}_1, \ldots, \mathrm{T}_k, \mathrm{M}_1, \ldots, \mathrm{M}_l \rangle$, where $\mathrm{Th} \cup \mathrm{Ob}$ is a non-empty class, $\mathrm{Th} \cap \mathrm{Ob}$ is empty, the O_i's are either classes or classes of n-tuples of members of Ob, the T_i's are either classes or classes of n-tuples of members of Th, and the M_i's are either classes or classes of n-tuples of $\mathrm{Th} \cup \mathrm{Ob}$. If O_i is a one-place predicate, then O_i is a class; if O_i is an n-place predicate then O_i is a class of n-tuples; similarly for the

T_i's and M_l's. Th is assigned to *Th* and Ob to *Ob*. The notion of the *truth* of a sentence S in $\mathscr{L}$ with respect to a possible model is defined in the usual way. A possible model is a *model* of a set of sentences of $\mathscr{L}$ if and only if every sentence in the set is true in $\mathscr{L}$ with respect to that possible model. We now have the following as a theorem:
Th. 1. Let $A = \langle Th_1 \cup Ob_1, Th_1, Ob_1, O_1, \ldots, O_j, T_1, \ldots, T_k, M_1, \ldots, M_l \rangle$ be a model of $\mathscr{T}$ such that for some non-empty T_i, some element of Th_1 is not in T_i if T_i is a class or not a component of a n-tuple in T_i if T_i is a class of n-tuples. Then there is a relational system $A^* = \langle Th_1 \cup Ob_1, Th_1, Ob_1, O_1, \ldots, O_j, T_1^*, \ldots, T_k^*, M_1^*, \ldots, M_l^* \rangle$ which is also a model of $\mathscr{T}$ and is such that $T_i \neq T_i^*$.

Proof: Let T_i be the class which does not contain some element, say q, of Th_1. Let r be an element of T_i (or a component of some n-tuple in T_i) which is distinct from q. Let ϕ be a one-one function of Th_1 onto itself such that $\phi(r) = q$. We define the function ψ as follows. If $x \in Th_1$, then $\psi(x) = \phi(x)$; if $x \in Ob$, then $\psi(x) = x$. A^* is now formed by replacing the elements of the classes (or components of classes of n-tuples) in the sequence A by their images under ψ. Since ψ is a one-one mapping of $Th_1 \cup Ob_1$ onto itself, A and A^* are isomorphic. Hence, since A is a model of $\mathscr{T}$, A^* is a model of $\mathscr{T}$. Furthermore, as can be seen from the construction, $T_i \neq T^*_i$.

The effect of *Th. 1* is to demonstrate the existence of alternate *physical* models for any theory $\mathscr{T}$ which is reconstructed along the lines given here. For if Ob_1 and Th_1 are taken as classes of observational and theoretical entities, then A^* will be an alternate physical model of $\mathscr{T}$. Notice that in the construction of A^* the interpretation of the observation terms is not altered, and the observational relata of the 'mixed' classes (the M_l's) remain as before. But at least one theoertical term (and for most theories, all of the theoretical terms) has received a different interpretation. From this result it follows that no amount of adding correspondence rules to a theory can ever result in 'pinning down' a unique physical interpretation for the theoretical terms of the theory.[1] So if a theory involving sub-microscopic particles, say, were reconstructed along the lines given above, the axioms (however numerous) would never specify *the* class of electrons, or *the* class of neutrons, for example. There would always be, given one true interpretation of the theory, another true interpretation in which the term 'electron' would now apply to some entities which were previously called 'neutrons'.[2]

[1] The addition of postulates may, of course result in the explicit definability of the theoretical predicates in terms of observation predicates, but this would result in violating the condition that the theoretical and observational entities are distinct.

[2] The construction would proceed along the lines of the proof of *Th. 1*. We would assume that, under the initial model for the theory, 'electron' is not assigned to the class of all of the theoretical entities, and that the classes assigned to 'electron' and 'neutron' are

But as far as Th.1 goes, the theoretical terms still retain a physical interpretation. The next theorem assures us of the existence of a true *numerical* interpretation for the theoretical terms, if there is a true physical interpretation of $\mathscr{T}$.

Th. 2. Let $A = \langle Th_1 \cup Ob_1, Th_1, Ob_1, O_1, \ldots, O_j; T_1, \ldots, T_k; M_1, \ldots, M_l\rangle$ be a model of $\mathscr{T}$ such that Th_1 is non-empty. Then there is a relational system.

$A^* = \langle Ar_1 \cup Ob_1, Ar_1, Ob_1, O_1, \ldots, O_j; T_1^*, \ldots, T_k^*; M_1^*, \ldots, M_l^*\rangle$ such that A^* is also a model of $\mathscr{T}$ where Ar_1 is a class of arithmetical entities.

Proof: Let Ar_1 be a class of numbers of the same cardinality as Th_1 (by the numeration theorem, Ar_1 might be the ordinal of the same cardinality as Th_1). Thus, there is a one-one correspondence, ϕ', between Th_1 and Ar_1. A^* is now constructed as in Th.1, using ϕ' for ϕ. Again, A and A^* are isomorphic. Hence A^* is a model of $\mathscr{T}$.

It follows from Th.2 that if a theory $\mathscr{T}$ has a model which provides an empirical interpretation for the theoretical terms, there is another model which assigns classes of numbers to the theoretical terms of $\mathscr{T}$. Again, the interpretation of the observation terms remains the same as do the observational relata of the mixed terms. In short, theories reconstructed within the framework given here cannot serve to characterise theoretical entities as physical, correspondence postulates notwithstanding. To see why this is so, it might be well to take a closer look at the correspondence postulates themselves.

I suspect that the main reason for the continued plausibility of the doctrine of partial interpretation of theoretical terms is a failure to recognise its incompatibility with the assumption that the theoretical and observational entities are always distinct, i.e. are disjoint classes. This can be seen from a simple example. Let 'T_1' be a theoretical term and 'O_1' an observation term (with a fixed interpretation). Now consider the following sentence:

$$(x)(T_1x \supset O_1x) \tag{1}$$

Now the claim is typically made for sentences such as (1) that, since 'O_1' is assumed to have a fixed interpretation, (1) partially interprets 'T_1'; (1) tells us that 'T_1' refers to *some* subset of the O_1's, even though (1) in itself does not single out any particular subset (cf. [6], p. 53). Notice that coming up with any (non-empty) numerical interpretation of 'T_1' is, of course, out of the question if (1) is to be true. Since we have assumed that

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disjoint. Then let the class assigned to 'electron' be T_1, and let q in the proof be any member of the class assigned to 'neutron'. The result is a model of the theory in which the term 'electron' now contains in its extension at least one entity (q) which was originally called a 'neutron'.

O_1 is a class of observational entities and T_1 is, by (1), a subset of O_1, T_1 must be a class of physical entities, and, in particular, observational entities. But it is here that we have the incompatibility with the assumption that the theoretical and observational entities are distinct; sentences of a form such as (1) can never serve to partially interpret theoretical terms, because, if they are to be true, then classes of theoretical entities must be subsets (or super-sets) of classes of observational entities. Actually, for reconstructed theories of the sort under consideration here, the correspondence rules would probably be similar in form to:

$$(x)(Th_x \,.\, T_1\, x \,.\, \supset (\exists y)(Ob_y \,.\, O_1 y))^1 \qquad (1)'$$

Now that (1) has been replaced by (1)', it can be seen that (1)' allows for the pernicious juggling of interpretations as in theorems 1 and 2 above. We can now tamper with the interpretations of the theoretical terms and not disturb the interpretations of the observation terms, and this is only possible if we assume the distinctness of observational and theoretical entities. Once this assumption is made theoretical terms are then most naturally defined as those which designate classes (or properties) of theoretical entities (elements of Th) only; observation terms are those which designate classes (or properties) of observational entities (elements of Ob) only; and the mixed terms designate classes of theoretical or observational entities (elements of $\mathrm{Th} \cup \mathrm{Ob}$). Now it is often the case that the theoretical term—observation term distinction is not drawn so as to rule out the possibility that theoretical terms and observation terms apply to the same objects.[2] I shall call a distinction which does not rule out this possibility a 'theoretical$_1$ term'—'observation$_1$ term' distinction. In this case, the objection against correspondence rules such as (1) does not apply in general, for in my terminology, these would specify partial interpretations of *observation terms*. But the objection can be raised all over again for those of the theoretical$_1$ terms which are theoretical terms in my sense (e.g. 'electron', 'neutron', etc.). For these terms, correspondence rules such as (1) again won't do, and surely such terms would occur in any plausible reconstruction of most physical theories.

But if all this be granted, and one is sympathetic to the realist account of scientific theories, there must be something amiss somewhere. One possibility is to require that the theoretical entities be restricted to entities which are in space-time, thus ruling out numbers as possible candidates. But if we are not to abandon the notion of the implicit definition of theoretical terms altogether, then this is no significant restriction on theoretical

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[1] The necessity for adopting a form such as (1)' was first pointed out to me by Grover Maxwell, and is implicit in (1).

[2] Pap ([6], p. 56) and Carnap ([1], pp. 47-48) rule this out; Hempel ([4], p. 78) does not.

entities at all. For to say that a set of entities are in space-time would merely amount to asserting that the set satisfies the *geometrical* axioms of our theory, and it follows from *Th.* 2 that a suitable class of numbers will do this for *any* set of axioms.

There is, I think, a more plausible reason which might be given as an explanation for the result given here. Theoretical entities are certainly considered as having *causal* connections with observational entities. Now the language $\mathscr{L}$ in which the theory was formulated was assumed to be extensional,[1] and perhaps our result again gives evidence to the claim that an extensional language $\mathscr{L}$ cannot 'capture' the sense of a causal connection. This contention is impossible to evaluate with any assurance, however, until a plausible and systematic account of the causal modalities appears.[2]

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4. Carl G. Hempel, 'The Theoretician's Dilemma" in Feigl, Scriven, and Maxwell, *Minnesota Studies in the Philosophy of Science*, vol. ii.
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[1] $\mathscr{L}$ was also first-order. But it is easy to show that *Th.*2 (and *Th.*1) holds for all higher order languages as well.

[2] As an example of the beginning of such a program, cf. 2, pp. 208-15.

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Discussions

MICHAEL FARADAY : A BIOGRAPHY

Professor Kuhn's criticisms[1] deal with three areas: on the most elementary level, he finds that I have either mis-read, misunderstood, or neglected certain basic scientific texts. He also questions my attribution of certain of Faraday's ideas to the influence of Kant and Boscovich and, finally, he notes that the sociological and the psychological dimensions of Faraday's life are almost entirely absent. There is, I think, a discernible bias running through all these criticisms of which even Kuhn may not be aware, and this is my reason for replying to his review. There are also some real areas of disagreement which may affect the future of the history of nineteenth-century science and which, I feel, deserve to be made quite explicit.

The charge of inaccuracy or 'careless reporting' is easily met for with one exception it is Kuhn who is in error. His 'pure' example of Ritter's double V tubes may serve here to illustrate this. He need not have gone back to Ritter's original paper to 'discover' the acid in the V tube; this is clearly stated on page 228. Indeed, the description of the experiment is exact. Kuhn's 'solution', incidentally, happens to be wrong. He feels that the presence of acid (in this case, sulphuric acid) removes all difficulties, both for the modern reader and for Ritter's contemporaries. Sulphuric acid, however, in the early 1800s was *not* H_2SO_4 but SO_3 and could not, therefore, have supplied the hydrogen. Georges Cuvier, in his report on Ritter's experiments to the Académie des Sciences, wrote: '[Galvanic action] decomposes the water, permitting the disengagement of one of the gases at the end of the wire, and conducting the other gas in some invisible manner to the end of the other in order to release it there;' (quoted on p. 230). If this statement is applied to Ritter's experiment, which Cuvier certainly knew, it necessarily implies the passage of the hydrogen through the connecting wire of the V tubes.

The other 'errors' Kuhn finds are of exactly this sort. On Volta, Kuhn neglects to inform his readers that there is a direct quotation from Volta which says precisely what I said Volta said. This quotation also explains the figure mentioned by Kuhn. It might be noted parenthetically here that the figures of which Kuhn complains are, page 152, an exact copy of the figure used to illustrate Wollaston's theory; page 157, an exact copy of Faraday's sketch. His trouble with primary sources here merely means that they *are* sometimes hard to follow. The figure on page 425 is a difficult one, and I must apologise for its obscurity.

Again, on Faraday's enunciation of Ohm's law, Kuhn is less than frank with his reader. Faraday says a good deal more than, 'the tendency to generate a current must be directly as [the conducting] power' and what he says is given in a quotation on page 210. It may be summarised as follows: given equal inducing (or electromotive force) the currents produced by magneto-electric induction in bodies are *exactly* proportional to their conducting power, with conducting power defined as the inverse of what Faraday calls the obstruction. I leave it to the reader to decide if this is Ohm's law.

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[1] This *Journal*, 1967, **18**, pp. 148-154.

I plead guilty to the error on Ampère. I should not have stated that 'the rationale of these experiments was clearly given by Ampère's theory', but, rather, that the rationale could be deduced from Ampère's theory, provided Faraday did not insist upon the absolute distinction between electrostatic and electrodynamic effects. The evidence presented on page 173 indicates that Faraday was not as conscious of this distinction as is Kuhn.

Kuhn further charges me with neglecting the works of contemporaries, such as Poisson, and here again I shall plead guilty. This time, however, my guilt is consciously self-imposed. I also did not discuss the mathematical aspects of Ampère's theory, the magnetic work of Gauss and Weber, and a whole host of other topics. My reasons are simple and, I would have thought, acceptable to Kuhn. I set out to write a biography of Faraday and the problem I set myself was to get inside Faraday's head, to see the questions that presented themselves to him, and to try and discover how he went about attacking and answering them. My discussion of the works of others, therefore, was limited to that part of it which Faraday read and understood. The problem of the nature of the voltaic cell, for example, upon which Kuhn places such emphasis is nowhere to be found in Faraday's early musings. Similarly, Poisson's achievement, of which Kuhn accuses me of being unaware, is irrelevant to a consideration of Faraday's intellectual development. Thus, I would maintain, that the 'pattern' which I am accused of reversing is, in fact, the same throughout the book. It is the pattern of Faraday's mind in so far as I could capture it. What I have not done is written a history of electricity and magnetism in the nineteenth century and Kuhn taxes me for this. I can only ask him to have patience for it is a future project.

The problem of the philosophical influences on Faraday is, as I have admitted in the book, a difficult one. My treatment has been uniformly criticised because of the paucity of evidence. All I can do here is point out that my hypothesis of the influence of Kant and Boscovich does serve to give coherence to Faraday's work where none was ever discerned before. As Charles Darwin wrote, 'In scientific investigations it is permitted to invent any hypothesis, and if it explains various large and independent class of facts it rises to the rank of a well-grounded theory' (C. Darwin, *The Variation of Animals and Plants under Domestication*, 2 vols., London, 1868, 1, 8). I claim no more for my hypothesis, hoping that further evidence will either confirm or refute it. May I, incidentally, suggest here that the fact that *Naturphilosophie* 'is now increasingly recognised as a vital formative element in nineteenth century scientific thought' owes something to my treatment of its influence on Faraday.

My case for the influence of Boscovich on Faraday rests upon far more than Kuhn states. My 'failure' to mention his belief in point atoms before 1844 simply does not exist. There is an extended section on Davy and Boscovich, drawn from Davy's writings; there is mention of Faraday's early enthusiasm for Thomas Thomson's textbook of chemistry in which Boscovich is given extended treatment; there is Faraday's mention of the theory of point atoms in his early chemical lectures (1819). Since I am in the process of preparing a paper on this topic, I shall not here spell out my arguments and further evidence for the connection between Faraday and Boscovich's ideas. There is certainly more in the book than Faraday's 1844 statement.

Finally, Kuhn objects to my failure to provide a sociological and psychological environment for Faraday. He appears to assume that Faraday *must* have been influenced by his milieu and by his friends. I agree with Kuhn that one should look for such a sociological medium for it *may* be important in the understanding of a man's thought. It is, after all, in the sociological context that the full force of the Kuhnian paradigm may be brought to bear. What Kuhn appears unwilling to concede is that there may *not* be a sociological context. Faraday was a loner. His scientific acquaintances were just that—acquaintances who were never admitted to the intimacy in which Faraday lived with his Sandemanian brethren. If Kuhn says that this point does not emerge in the book, I can only take his word for it, but the point is there although it is not insisted upon. Where there is evidence for influence, as with Davy and John Herschel, the influence, I maintain, is clearly delineated.

The degree to which the book captures the essence of Faraday's personality will have to be decided by each reader. I think the key phrase in Kuhn's critique here is 'plausible to a twentieth century reader'. The evidence we have of Faraday's personality reveals him as kind, serene, proud, gentle, honest and simple, in the Victorian sense of the word. These are without exception the qualities attributed to him by his contemporaries. Since Freud, of course, it is impossible to believe that anyone could *really* be like this and so we are expected to detect the seething volcano which *must* exist under this calm exterior. I classify psychoanalysis with humoral pathology and would as soon describe Faraday as phlegmatic or jaundiced as set off on a psychoanalytic voyage *for which there is no guide whatsoever* in the sources. Nor is Kuhn much help here. How, he asks, can the simple man I have described be 'also the man who was repeatedly concerned to establish his priority in scientific discovery'. There is no problem at all, for Faraday never was repeatedly concerned to establish his priority. In 1821 and 1823 Faraday wished to establish his claim for originating the idea of electromagnetic rotations because he had been accused of plagiarism. In 1831, Nobili and Antinori published an account of electromagnetic induction experiments actually based on Faraday's great discovery. There were some, however, who did not read carefully and thought that Nobili and Antinori had discovered electromagnetic induction before Faraday. Faraday was quick to point out that he was the source so that another charge of plagiarism could not be made. I do not know the degree of complexity necessary to be sensitive to the accusation of plagiarism but I suspect it is not large. That Faraday was not obsessed by priorities is easily shown: in 1845, for example, when informed that others before him had discovered the peculiar action of bismuth in a magnetic field, he added a note to his paper giving them credit. This example could be multiplied. Indeed, these incidents seem to me to buttress the very depiction of Faraday's character given in the book. He may have been concealing a volcano but even Kuhn must confess that he did a wonderful job of it—so good, in fact, that there is not a shred of evidence to indicate that it existed.

This rather detailed rebuttal of Kuhn's criticisms was not solely intended as a defence against the serious charges of sloppy scholarship which Kuhn has levelled against me. It was also meant to point out the position which Kuhn has assumed—one which follows almost automatically from his well-known philosophical views. Kuhn, it seems to me, is essentially hostile to the idea of biography as the proper mode of the history of science. The scientific maverick (and many of the truly creative scientists seem to me to fall in this class) is immune to the siren song of the paradigm. I think Kuhn's disappointment with my biography of Faraday comes, in large part, from the fact that no matter how hard he tried to push Faraday into the scheme which he feels reveals the true dimensions of the history of science, Faraday would not fit. Thus the pique at the failure to discuss the Royal Institution as a unique scientific institution, and the sharp language on the failure to sketch in the voltaic and electrodynamic paradigms and so on. If there are dangers in the biographical approach as I have used it, it seems equally clear that there are also dangers in the kind of history of science that Kuhn wishes I had written but did not.

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L. PEARCE WILLIAMS

Professor Kuhn replies:

I am grateful for Professor Williams's attempt to clarify the issues that divide us. If it leads readers to examine his book and reach their own conclusions, both of our purposes will have been achieved. I shall here only insist that, where particular passages of his book are at issue, our differences are not at all the product of misreading or of incomplete reporting in my review.

POPPER ON LAW AND NATURAL NECESSITY

1. In Appendix *10 of *The Logic of Scientific Discovery*,[1] Professor Sir Karl Popper, following upon an earlier treatment,[2] has sought to define the notion of a law of nature, which is characterised by 'natural or physical necessity' (p. 428). Such a law is said to be (1) 'logically stronger' than just a true, strictly universal statement (pp. 426, 431, 432). For example, suppose that every member of the (now extinct) New Zealand moas died before the age of fifty; that 'moa' is taken to be a 'universal name'; and finally that omnitemporally there are no other moas. Then it is a true, strictly universal statement (call it '*M*') that 'All moas die before the age of fifty'. But, supposing, for example, that the deaths before fifty of all moas were due to the presence of certain viruses, *M* would not be called a law of nature, but rather a merely accidental universal (pp. 427 f.). This concedes (for example) William Kneale's argument that the characterisation of a law, from a

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[1] This is one of the additions to the original *Logik der Forschung* included in the English translation (London, 1959), to which all page numbers in the text refer.

[2] 'A Note on Natural Laws and so-called "Contrary-to-Fact Conditionals",' *Mind*, **58** (1949), 62-66.

common Humean standpoint, as just a true, strictly universal statement is neither logically sufficient nor intuitively adequate (p. 427). Nevertheless, as against Kneale, Popper holds that (2) laws of nature are not logically necessary; 'compared with logical tautologies' they are contingent, accidental (pp. 429, 432, 438). On Professor Popper's positive account, 'a statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds that differ from our world, if at all, only with respect to initial conditions' (p. 433—original italicised).

2. But this definition cannot be taken quite literally, for as it stands it does not afford a basis for any differentiation at all among the universal propositions true of our world. The device of a range of worlds is to eliminate some of these universal propositions, viz. certain empirical strictly universal ones, as not physically necessary, because false in at least one among the range of worlds. Yet the limits of the range are specified as those which 'differ from our world, if at all, *only* with respect to initial conditions' (our italics). But a world which differs from ours in respect of any empirical strictly universal proposition such as *M* does not differ only with respect to initial conditions. *M* is not a statement of initial conditions. Hence, a world in which *M* can be false is one outside the range of worlds which the definition provides. Therefore, despite Popper's claim for his example, the proposition *M*, not being a statement of initial conditions, is true in all the worlds thus specified, and *a fortiori* it is deducible from a statement function which is satisfied in all such worlds; therefore is it not excluded from physical necessity by the definition in question.

3. However, what would seem to be the import of the definition may be brought out in the following way. Consider *M* again. Call a statement about the presence of certain viruses, which formulates relevant initial conditions, '*C*'. Then we may assume that *M* follows from the conjunction of *C* and some set of general statements about the structure, functioning, evolution, etc., of organisms, which we may call '*L*'. Then *M* is said not to be a law of nature, because its truth is not invariant with respect to change in initial conditions (presence versus absence of viruses). But *L* is, presumably, a genuine law, because its truth is so invariant: whatever the special conditions of life organisms are subject to the conditions stated there. We suggest then that the intent of the definition may be expressed as follows: a statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds which instantiate the properties and relations of our world but which may differ from our world with respect to initial conditions.

4. Now what, more exactly, is the possible range of variation of the initial conditions? *A priori* there would seem to be two and only two alternatives. One is that the freedom to vary initial conditions stops short only at those embodying recognisably self-contradictory forms. But a consequence of this would be that laws of nature are logically necessary; in so generously provided a range of worlds we should be able to find singular propositions which are contraries of every logically contingent universal proposition. Put otherwise, if '*p* is a law' entails that *p* is deducible from a statement function which is satisfied in all worlds which instantiate the properties and relations of our world whatever the (logically possible) initial conditions, then if *q* formulates certain initial conditions, and *p* and *q* together entail *r*, *r* must of necessity hold in the 'world' so described.

But if p is genuinely contingent, it must be possible that, though p and q together entail r, nevertheless in fact q and not-r. In brief, this interpretation yields a position indistinguishable from Kneale's.[1] On this ground alone it may be concluded that this cannot be the intent of Professor Popper's definition.

5. In addition Popper says that 'natural laws . . . restrict the (logically) possible choice of singular facts' (p. 430). This brings us to the second of the two possible alternatives regarding the range of permitted variation of initial conditions, viz. that the class of possible initial conditions is the class of physically (rather than logically) possible ones. Now it is generally agreed that a physically possible condition is one which is not inconsistent with a physically necessary statement, or law of nature. But this restriction on the class of possible initial conditions makes the definition (both original and revised) clearly circular. For it would now read: a statement may be said to be naturally or physically necessary if, and only if, it is deducible from a statement function which is satisfied in all worlds which instantiate the properties and relations of our world but which may differ from our world with respect to physically possible initial conditions, i.e. initial conditions which are not inconsistent with a naturally or physically necessary statement.

6. In sum, we have argued three points with regard to Professor Popper's definition of a law of nature, or of natural or physical necessity: (1) that it is at least verbally defective, as imposing, as it stands, no distinctions on the class of universal propositions true of our world; (2) that a version verbally revised in this respect may be interpreted, at a crucial point, in only two possible ways, one of which renders it equivalent to a view which Professor Popper wishes to reject; and (3) the other renders the definition circular.

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CONNECTIVITY, CHANCE, AND IGNORANCE

An argument, L, is presented in section 4 of my 'Connectivity, Chance, and Ignorance', this *Journal*, **16** (1965), 214-15, [1], which purports to derive equality of chances of heads and tails for an unbiased coin from a principle of connectivity. L fails, as Professor I. Levi has pointed out to me, because of an unclarity as to what chance proposition states the 'single physical property' of the coin tossing set-up to which connectivity is applied. In what follows, the fallacy is displayed and a valid argument for the same conclusion is presented. The nomenclature of the original argument is at once cumbersome, out of accord with recent literature, and largely confined to the fallacious section of the paper. I therefore replace it here with a more convenient notation.

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[1] Kneale has in fact claimed that Popper has committed himself to just this position which he intends to reject. See 'Universality and Necessity' this *Journal*, **12** (1962), 99, 101, and (with M. Kneale) *The Development of Logic* (Oxford, 1962), pp. 650 f.

Consider two coin tossing set-ups A and B (S_1 and S_2 in [1]), which do not differ in any non-chance property connected to (i.e. whose presence or absence influences) the chances of heads or tails. Then the principle of connectivity is taken to assert that A and B cannot differ in their chances of heads. But it is not made clear in [1] whether the chance of heads in a set-up is a property distinct from its chance of tails. Either answer to this question shows the argument L to be invalid.

First, L invokes a distinction between the set of properties of A 'peculiar to heads' and the set of those 'peculiar to tails'. For anything to follow from the connectivity principle, the phrase 'peculiar to heads' must be taken to mean 'connected to the chance of heads', and similarly for 'tails'. But now make in turn the alternative assumptions that there (1) is not, (2) is, a third possibility (for example, the coin landing on its edge):

(1) If heads and tails are exhaustive as well as exclusive outcomes, the probability calculus requires the sum of their chances, $p_A(H)$, $p_A(T)$, ($P_1(h)$, $P_1(t)$ in [1]) to be unity. Thus the value of each is uniquely determined by the value of the other, and the sets of properties connected to each must be identical, which contradicts an assumption of L.

(2) If heads and tails are not exhaustive outcomes, the value of $p_A(H)$ does not uniquely determine that of $p_A(T)$ (though it will of course set limits: if $p_A(H) = p_{A1}$, then $0 \leqslant p_A(T) \leqslant 1 - p_{A1}$). Then some non-chance property of A may indeed be connected to tails without being connected to heads. For let the chance of any other outcome be $p_A(E)$. Then suppose, for example, that deforming the coin in some way increases the value of $p_A(E)$ at the expense of that of $p_A(T)$, leaving $p_A(H)$ unaffected. In this case, the sets of properties connected respectively to the chances of heads and tails can be distinct, as L requires. But just in this case, and for this reason, the chance of heads and the chance of tails may be taken as distinct properties of A, *and may therefore be connected to each other*. But if so, then a difference between the chance of heads on set-ups A and B could be explained by corresponding differences in the chances of tails, and all the *non-chance* properties of A and B could be identical. Hence for connectivity to apply, the set of properties connected to the chance of heads must be able to contain the chance of tails. But the conclusion of L then becomes trivial, since a difference in the chances of heads and tails in one set-up can now be explained by the difference in the chances of tails and heads! As an argument for the desired conclusion, which requires such a difference to be accompanied by an asymmetry in *non-chance* properties, L is invalid.

Thus L fails on either of the exhaustive assumptions (1) and (2). A valid argument for the desired conclusion exists, however, and runs as follows. The single physical property of the set-up A, to which other properties may be connected, is the chance distribution D_A over all the outcomes. As full generality is given by three outcomes, the chance of any outcome other than heads or tails is represented by '$p_A(E)$' as above. Then D_A may be represented by an ordered triple, of the values p_{A1}, p_{A2}, p_{A3}, of $p_A(H)$, $p_A(T)$, $p_A(E)$, respectively, i.e.

$$D_A =_{df} \langle p_{A1}, p_{A2}, p_{A3} \rangle \tag{1}$$

And similarly for the set-up B.

Next, let S_A be the conjunction of all true propositions ascribing to A properties connected to D_A, and S_B be the proposition obtained from S_A by substituting 'B' for 'A' throughout the sentence expressing S_A. Then the principle of connectivity asserts that

$$S_A \equiv S_B . \rightarrow . D_A = D_B \tag{2}$$

The status of '$\rightarrow$' reflects that of connectivity which, as a regulative principle, it is unprofitable to classify as either analytic or synthetic. (2) merely expresses the fact that acceptance of the principle licences inferences from '$S_A \equiv S_B$' to '$D_A = D_B$' and from '$D_A \neq D_B$' to '$S_A \not\equiv S_B$', while leaving open the question of the status of such inferences.

It follows trivially from (1) and (2) that

$$S_A \equiv S_B . \rightarrow . \ p_{A1} = p_{B1} \tag{3}$$

Now the crucial, and true, assumption of L may be introduced exactly as in [1]:

> Those physical differences which serve to label one side of the coin 'heads' and the other 'tails' are certainly not in the set of properties by which a difference of chance is to be explained. If they were, if labelling the sides of a coin generally biased it, the explanation of the bias would be trivial, and a coin would be unacceptable as part of a chance set-up. In short, these differences are not taken as [connected] physical properties of the set-up.
>
> It follows that the application of connectivity is unaffected by relabelling the sides of the coin in, say, [B]. The condition for qualitative identity of [A] and [B] would then be that every true proposition ascribing a [connected] physical property to [A] should become true of [B] when 'tails' is substituted for 'heads' (and vice versa) wherever the latter occurs in the sentence expressing the proposition. For example, if the coins are magnetised and the N-pole in [A] is heads, in [B] it must be tails. If this condition is satisfied, connectivity requires that the chance of [A] falling heads shall be the same as that of [B] falling tails.

In the present terminology, this assertion is expressed as follows. Let S'_B be the proposition obtained from S_B by carrying out the described substitution of 'tails' for 'heads' (and vice versa) in the sentence expressing it. The same substitution in the sentence expressing the proposition that the chance distribution in B is D_B will clearly yield a sentence expressing the proposition that the chance distribution in B is D'_B where, D'_B is obtained from D_B by interchanging the values of $p_B(H)$ and $p_B(T)$, i.e.

$$D'_B = \langle p_{B2}, p_{B1}, p_{B3} \rangle \tag{4}$$

Then applying the principle of connectivity to the set-ups A and B, after the labelling in B has been reversed, yields the assertion that

$$S_A \equiv S'_B . \rightarrow . \ D_A = D'_B \tag{5}$$

Again, it follows trivially from (1), (4) and (5) that

$$S_A \equiv S'_B . \rightarrow . \, p_{A1} = p_{B2} \tag{6}$$

From this point, the argument is parallel to that given in L (1)−(7): Suppose that (3) is exemplified by A and B, i.e. that

$$S_A \equiv S_B \tag{7}$$

Hence from (3) and (7)

$$p_{A1} = p_{B1} \tag{8}$$

Suppose moreover that

$$p_{B1} \neq p_{B2} \tag{9}$$

(e.g. $p_A(H) = p_B(H) = 0{\cdot}7$)

Hence from (8) and (9)

$$p_{A1} \neq p_{B2} \tag{10}$$

Hence from (6) and (10)

$$S_A \not\equiv S'_B \tag{11}$$

Hence from (7) and (11)

$$S_B \not\equiv S'_B \tag{12}$$

Thus *if* a set-up A exists related to B by (7), and the chance of heads in B differs from the chance of tails in B, *then* the truth value of the conjunction of propositions ascribing connected physical properties to B is changed by the substitution of 'tails' for 'heads' (and vice versa) in the sentence expressing it. In other words, under these conditions, connectivity requires there to be an *asymmetry in some non-chance property* of B, i.e. roughly, that some non-chance property truly ascribed to heads in B is falsely ascribed to tails.

Now, of the two conditions, the existence of the set-up A is guaranteed by the existence of B, since the latter is itself such a set-up, whose self-identity entails the truth of (7). We may thus conclude that, for any coin tossing set-up, connectivity requires a difference between the chances of heads and tails to be connected to an asymmetry in some non-chance property. Conversely, if the coin is unbiased in the sense that there is no such asymmetry, then connectivity asserts that there is no difference either between the chance of heads and the chance of tails. If the coin must fall either heads or tails, then we arrive again at the proposition p_1 of [1],

'The chance of an unbiased coin falling heads is 1/2.'

Thus the object of section 4 of [1] is attained, though not by the argument there given. The argument of the rest of the paper then proceeds to establish equal prior probabilities in certain cases on the basis of these chance propositions, without relying on ignorance or indifference principles. The importance of the argument here presented is that it is a necessary stage in that larger argument. There is a close connection between this use of connectivity to establish equal prior probabilities and Hacking's application of a fiducial argument to establish corresponding posterior probabilities (*Logic of Statistical Inference*, pp. 137-49), which I think shows Hacking to be wrong in supposing that he has no commitment to prior probabilities; but that is matter for another paper.

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REFERENCE

[1] Mellor, D. H., 'Connectivity, Chance, and Ignorance', *Brit. J. Phil. Sci.* **16** (1965), 209–25.

THE WHITE SHOE: NO RED HERRING

In a recent note,[1] Dr I. J. Good argues that the paradoxes of confirmation are spurious on the ground that one of the two assumptions underlying them is false. I wish to show that Dr Good's argument fails to establish his point.

The paradoxes of confirmation[2] follow deductively from two assumptions: (*a*) Whatever confirms a given hypothesis also confirms any logically equivalent one; (*b*) A hypothesis of the form 'All F are G' is confirmed by any of its instances, i.e. by any object that is F and also G; or rather, by any sentence of the form 'i is F and i is G', where 'i' is a name of some particular object.

Reasons for construing a hypothesis as confirmed or disconfirmed by evidence sentences rather than by objects were offered in section 6 of my article in *Mind*; to these, there may be added the consideration that one and the same object may have properties that make it confirmatory, and others that make it disconfirmatory for a given hypothesis. A particular bird may be a crow and black, but may also have an albino crow for a sister; in virtue of these properties, it would both confirm and disconfirm the hypothesis 'All crows are black'. This consideration suggests that an object can be said to confirm or to disconfirm a hypothesis only *under a particular description*, and it is descriptions, therefore, that should be counted as confirmatory or as disconfirmatory for a hypothesis.

The two assumptions (*a*) and (*b*) imply the paradoxes of confirmation. For example, the hypothesis 'All crows are black', being equivalent to 'All non-black things are non-crows', is confirmed, *inter alia*, by a white shoe, or rather, by the evidence sentence 's is not black and s is not a crow', where 's' is a name of some white shoe. Dr Good argues that assumption (*b*), which he states in the form 'A case of a hypothesis supports the hypothesis', is simply false, and that, therefore, 'the white shoe is a red herring.'

To establish his point, Dr. Good considers two worlds, let us call them W_1 and W_2, about which the following information S is given: W_1 or W_2 is the world we are in. W_1 contains 100 crows, all of them black, and one million other birds; W_2 contains 1001 crows, of which 1000 are black and one white, plus one million other birds. One bird has been selected equiprobably at random from all the birds in our world. It turns out to be a black crow.—This information, Dr Good argues, provides strong support (in a technical sense which need not be considered here) for the hypothesis that we are in world W_2, where *not* all crows are black; and he concludes: 'Thus the observation of a black crow, in the circumstances described, undermines the hypothesis that all the crows in our world are black.'

But whatever the merits of this argument may be, it clearly does not refute the assumption (*b*). For to do so, it would have to show that an evidence sentence of the form E: 'c is a crow and is black' considered by itself and without reference to any other information,[1] may fail to support the hypothesis that all crows are black; whereas Dr. Good's example concerns the confirmatory role, not of *E*, but of the vastly stronger evidence sentence *S* stated above. That an evidence sentence which contains *E* as one conjunctive component may be disconfirmatory for the hypothesis in question is perfectly obvious; the sentence '*c* is a crow and is black, and *d* is a crow and is non-black' will do. But assumption (*b*) concerns only the case where the given evidence consists exclusively of a sentence of the form *E*; and that this sentence must be taken to support the hypothesis seems to me undeniable. I conclude, therefore, that the white shoe is not a red herring, after all.

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[1] I. J. Good, 'The White Shoe is a Red Herring', this *Journal*, 17 (1967), 322.

[2] The paradoxes were stated briefly in my article 'Le problème de la vérité', *Theoria* 3 (1937) (specificially, p. 222) and were developed more fully in subsequent publications, among them 'Studies in the Logic of Confirmation', *Mind*, 54 (1945), section 5; this essay is reprinted, with a 'Postscript (1964) on Confirmation' in my book, *Aspects of Scientific Explanation*, 1965.

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[1] The crucial importance of disregarding additional information in judging the confirmatory relevance of a given evidence sentence for a hypothesis was emphasized in section 5.2(*b*) of my article in *Mind*, where it was pointed out that the very appearance of paradoxicality in cases like that of the white shoe results in part from a failure to observe this maxim.

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Reviews

Studies in the Philosophy of Charles Sanders Peirce, second series. Edited by Edward C. Moore and Richard S. Robin. (The University of Massachusetts Press, Amherst, 1964.)

This is a second volume of essays about Peirce and his philosophy. Edward C. Moore and Richard S. Robin have assembled it on the unintrusive policy of inviting contributions and then grouping them according to special subjects. This has led to a collection in which very different interests are expressed and in which, as the editors say, we are shown 'the situation in our understanding of Peirce today' (p. v). The less good consequences of their policy are that some of the studies are extremely bad, that many of them uselessly overlap, and that important topics in Peirce's thought are not studied in the book at all.

Perhaps the best way to organise a review of the collection is to divide the essays into these three groups: (1) those which do not assume a special acquaintance with Peirce, the biographical and expository essays; (2) those which suppose that we know a fair amount about him and which aim for deeper understanding of his views; and (3) those which assume that we already know enough to consider whether Peirce's views are sound.

I

Almost half the essays are in the first group. They will be appreciated differently depending on the interest one has in Peirce. For example, though I was surprised to learn from Lenzen how large and important a contribution Peirce had made to astronomy, I was disappointed that virtually nothing was said about the bearing of Peirce's scientific work on his philosophy. Others will, no doubt, not feel this as a loss, but then this is the usual variety in our response to expository articles.

In the first group, too, there are three careful studies of Peirce's logic, by Prior, Turquette and Roberts. They show, as careful studies of the logic always do, that Peirce was wonderfully inventive and that he anticipated many of our current views. Their distinctive point is the suggestion that some of Peirce's ideas in logic could still with profit be followed up. In fact, none of them does develop the ideas, but what is even more disappointing is that, though they are good, the essays are so conservative in logic they do not see what Peirce was most concerned to do. The oversight is plainest in Turquette's study; it is the most philosophically ambitious of the three.

Professor Turquette's study is about a part of Peirce's 1885 paper 'On the Algebra of Logic—A Contribution to the Philosophy of Notation'. This is the famous paper in which Peirce devised a notation for truth values and used it to decide whether forms of statements are necessary or not. According to Turquette, however, the 'current value' of the paper is not 'exhausted by the concepts of truth value and quantifier' (p. 96). The current value is that Peirce's icons of the algebra of logic are a logical system of such simplicity that they should encourage us to construct fresh systems which, like the icons, use material implication as a primitive. Modern logic has *perhaps* so far failed 'to follow more closely in the direction set by Peirce' (p. 104) because there have been doubts about the fundamental place of material implication. Nevertheless—this is Turquette's claim—since systems 'constructed in terms of material implication are so simple and elegant, there seems to be no really convincing reason for rejecting material implication as a basic logical operator'; besides, we 'could probably still learn a few lessons by cultivating the ability to assign new interpretations to logical expressions' through one of Peirce's devices, his fourth icon, which Turquette reads as a definitional schema (p. 106).

Turquette's study is generous in spirit but it misses the aim of Peirce's work. Peirce's essay is about notation, icons especially, but only once does Turquette come near to asking what Peirce thought his icons are iconic *of*, and then, immediately, he leaves iconicity aside as if it were irrelevant. An icon, Peirce says, is the kind of sign that is taken to be a sign because it resembles what it is used to signify. Peirce thought there had to be icons in logic. What did he think they pictured? The answer is that icons picture 'the essential and elementary type of mental movement' (unpublished in collections of Peirce's work, quoted on p. 104). Peirce came, as Turquette notes, to doubt that implication is really elementary and to claim that other signs are really the icons of logic. But without commenting on Peirce's programme, without deciding what the basic icons are or even whether notations are or should be iconic, Turquette goes on to recommend an amended icon set. There are 'advantages to logical systems' set out from other basic signs, he says, 'but this does not detract from the merit of systems like Peirce's which are based on a sign for material implication . . . such a choice of a basic sign can lead to a simple and elegant set of axioms like that of Tarski-Bernays' (p. 105). The merit, then, has nothing to do with iconicity. So Peirce's aim in the very piece Turquette is studying has been left aside. It is overlooked or not taken seriously, I think, because it is too different from the interest of most logicians now.

At the start of the 1885 paper, Peirce said that he wanted 'to develop an algebra adequate to the treatment of all problems of deductive logic, showing as I proceed what kinds of signs have necessarily to be employed at each stage of development' (3.364 and quoted on p. 97).[1] We hear nothing from Turquette, nor much from anyone else, about the kinds of signs that have 'necessarily to be employed' in logic; nor is anything said about stages of development. These are not issues for Turquette or other logicians who are interested in the power and economy of logical systems. Then too, in the continuation of the passage just quoted, Peirce said that a third result of his programme will be to attain

> the enumeration of the essentially different kinds of necessary inference; for when the notation which suffices for exhibiting one inference is found inadequate for explaining another, it is clear that the latter involves an inferential element not present to the former. Accordingly, the procedure

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[1] The citations to Peirce in this review are sometimes given to the volume and paragraph number of *The Collected Papers of Charles S. Peirce*, and sometimes to the page in the *Studies* on which a quotation from Peirce appears.

> contemplated should result in a list of categories of reasoning, the interest of which is not dependent upon the algebraic way of considering the subject (3.364 but not quoted by Turquette).

Here, a difference in Peirce's and Turquette's interests is quite plain. For Peirce there is no question whether this or that can be proved, and no question whether this or that system is consistent, simple or complete or not. His questions are what are the formal features of this or that valid inference? What are the kinds of valid inference? For Peirce, the issue of what connective is basic is not the same as settling on basic operators in an axiom system. And one cannot be concerned both for stages of development and a notation which is uniform for all kinds of inference, regardless of their stage. What Peirce wants and what his icons provide is a way of reinterpreting in the light of higher developments the notations which are adequate for inferences of a less complicated kind.

Turquette's paper, then, does not attend to the questions in logic which Peirce thought he was answering (and something like this is missing in Prior's and Robert's papers too). They may not have been good or important questions, but it is of no use to ask what we can still learn from Peirce in logic unless we see what he was trying to do. The logicians who study Peirce nowadays are sympathetic, helpful and ingenious (again, I want to say how good the Prior, Turquette, and Roberts essays are), but they have little sight into the great differences between Peirce's focus in logic and the styles and practices of the present time. Peirce, for them, is a brilliant pioneer in the study of the systems they are themselves concerned to study now. They do not see that he was set upon a different course and had a more philosophic aim.

The remaining essays in the first group are more philosophical expositions. Two of them are especially likely to interest readers of this journal: Thomas Goudge's essay on Peirce's evolutionism and Idus Murphree's study of his experimentalism.

Goudge's question is how well Peirce's thought about evolution has stood up, whether 'recent scientific findings' 'verify' or 'refute' his views. The idea seems to be that Peirce's thoughts about evolution went beyond the scientific claims of his time, that some of them tried to anticipate scientific results, and that the anticipations can be checked with what has been learned more recently. Were Peirce's guesses right? Were they mainly wrong? These questions, I am afraid, are much too crude, for they do not appreciate how Peirce distinguished between scientific and philosophical ideas, or how he thought philosophy could be guided and corrected by the sciences. Goudge never gets these issues clear. Their confusion, unfortunately, mars both his otherwise well-organised exposition of Peirce's views and his own judgment that, except for the evolutionary cosmology, Peirce's ideas have stood up fairly well, that most of the (scientific? philosophical?) extensions he made of scientific notions do not have to be revised.

The reason for this assessment, I gather, is that Goudge thinks that Darwin was, after all, mainly right and that 'Peirce had a remarkably accurate grasp of Darwin's theory' (p. 323). He failed only to see a few important points and he was too generous in allowing for factors other than selection. Goudge thinks, for example, that Peirce did not understand how selective evolution could be creative. Peirce had argued that 'selection is exclusively an eliminative factor' and that 'it cannot account for the vast increase in diversity and the creation of new forms which have occurred during the history of life' (p. 325). According to Goudge, the failure here is that, though Peirce is right about selection's being eliminative, an eliminative factor, rightly understood, can account for diversity and creation. How? Goudge: in the way that creativity 'is applied to the activity of a sculptor or an architect' (p. 326, with supporting reference to G. L. Stebbins and G. G. Simpson). Peirce was led to his misunderstanding by 'his transcendentalism'; it was against selection, 'the naturalistic implications of Darwinism (were) unacceptable to him' (p. 326).

This accusation and explanation won't do; they aren't well enough thought through or on the point that Peirce had in mind. The relevant point for Peirce is what a theory of evolution is to explain, how wide or deep it is to be. If we first suppose that there are potentialities for novelty in species and then ask what accounts for the fact that these or those forms are shown, it is all right to talk exclusively of selection. But if we ask about the potentialities themselves, if we think of an evolutionary theory as saying something about them too, we have a different matter. The shape of the theory of evolution is at issue, not whether a point in *the* theory has been grasped. Goudge has not seen this, and my impression from his citations is that some 'present day evolutionists' (p. 326) have failed to see it too. To explain those features of evolution on which it focuses, Goudge is perhaps right that Darwinism does not need 'to be supplemented by non-selectionist theories' (p. 328). But to explain other connected features, of course it does. The other features were what Peirce was pointing to.

There are, Goudge reports, no canonical theories of cultural and cosmic evolution, so there is no question of comparing Peirce's thought with prevailing contemporary views. The few points about cultural evolution 'which seem to be fairly well established' are 'in line with what Peirce was saying over half a century ago' (p. 334). The same is true in studies of cosmic evolution, though here Goudge is suspicious of how far Peirce or anyone else makes sense. Peirce, he finds, is agreeably sensible even when he suggests that there has been an evolution of the laws of the universe. But when Peirce goes on to say that since time is continuous, it must be 'an organised something, having its law or regularity; so that time is itself a part of the universe whose origin is to be explained' (6.214, quoted on p. 337), Goudge thinks he has fallen into senselessness.

My impression is that Peirce's idea of time's having an origin has a good deal of sense, though of course it can be phrased foolishly. The question is whether there are aspects of time and, if so, which of them is basic or least affected by other sorts of things. In particular, it is whether regularity of transcience is a feature of time itself or is wholly or partly owed to something else. If there could be no time apart from other sorts of things, time will have to be affected by them; it will be *in* them or *of* them, or it will have its nature detailed or somehow modified by them. In either case, there will be derivative features in temporality, and we can ask about them without asking whether there was a time before which they came about. Indeed, we should ask about them to settle the range within which 'evolution' is an applicable idea.

Goudge, then, seems to me too hesitant about cosmic evolution. Peirce's ideas about the scope and content of a theory of evolution are pertinent contemporary concerns. His knowledge of the science of his day was full and accurate, and he had enormous critical and generalising power. Goudge sets out many of his ideas in an open and attractive style that lets us get at them. That's the value of his article; more, I think, than the comparisons with recent scientific views.

Idus Murphree's study is a defence of pragmatism's claim that reason is tested but not demeaned by practice and experiment. The parts of it that interest me most are the general remarks on the themes of reason and practice. I wish Murphree had gone into them more, for they have not been carefully enough explored in our philosophies of science. We know, of course, that hypotheses are tested in practice. But what we have not understood well enough is that testing changes them. It does not, as it were, merely say 'yes' or 'no' *to* ideas that are all definite before we act; it makes a difference *in* the ideas themselves, a modal difference, the difference between something's being only possibly true and its being claimed as or being true in fact.

Usually, in our logics of confirmation, this modal difference is either neglected or denied, and then modal terms are treated as an afterthought, as if the same proposition could both be possibly true and true in fact. The reason, I suppose, for saying that propositions are unaffected by their test is that we want to claim that an experiment confirms *what* we had thought before. But we pay a heavy price for preserving just *this* sameness in our thought. One part of the price is that we are unclear about the modality of the parts of the conditional proposition which occurs in a confirmation argument; we are unclear even about the modality of the whole conditional. The confirmation argument is that since p implies q, finding q or not q will partly confirm or disconfirm the p. The p which occurs in the conclusion is said to be actually true or not. But what is the modality of the p which occurs in the conditional? What is the modality of the conditional itself? And if the p in the conditional and in the conclusion have different modalities, how will the argument have justified the change? We can't read the conditional as saying either that if p is possibly true then q is possibly true or that if p is possibly true then q is actually true—for since all non-selfcontradictory propositions are possibly true, the 'if . . . then' will in neither case have a distinctive meaning. If we mean that if p is actually true, q is actually true, to claim that asserting q confirms what was beforehand merely possible, we will have to add 'p is possibly true' as a premiss in the argument. In turn, however, that will put p's of different modalities in the premisses themselves and the connection between the different p's will still have to be explained. We want modalities somewhere in a confirmation argument. It is hard, though, to see where they can be.

Perhaps our unclearness about the modalities can be made more evident by seeing that we have two vocabularies, that we use them both, but that we have not settled how they go together. On the one hand, we talk about propositions and the logical relations between them. On the other, we talk about what we believe or assert or are willing to submit to test. In the talk about propositions, there is no distinction between a proposition's being possibly true and true. The modal distinctions occur in the other vocabulary; they have to do with our knowing or not knowing about relations between propositions. The awkwardness comes when we say that certain arguments, certain propositions taken together, are our reasons for believing or asserting that what is possibly true is also true in fact. The awkwardness is that we have not settled how the believing or asserting is distributed through the propositions in the argument; we are unclear about connecting an account of the formal relations between propositions with what we want to say about the development of our assurance that, because of an experiment, this or that is sure or only likely to be true. The modal terms occur mainly in one of our vocabularies; they, or some counterpart of them, should occur in the other too.

There is a suggestion in Peirce which is very useful here. It is simply that hypotheses are themselves believed. Peirce's idea is that we do not start a course of reasoning with a proposition which is neither believed nor not and then come to a belief. Nor is our reasoning second intentional or on different levels; it does not start with a belief that a proposition is possibly true and then conclude the proposition or conclude that it is in fact true. Instead, the proposition to be tested is believed right at the start, and the belief is deepened or set aside in the course of reasoning.

Professor Murphree's exposition of this point is unfortunately a little flawed. He says that, for Peirce, inquiry begins 'devoid of belief' and that it concludes with what is 'now confirmed belief' (p. 236). These are ambiguous and unbalanced phrases (for a confirmed belief is a belief). They occur, I think, because Murphree does not bring forward the view of Peirce's which he emphasises early in his article, that abduction is an inference to an hypothesis, and that an hypothesis therefore is something we already have some reason to think is true. Were we to keep this view in mind, we could have an account of the confirmation argument which makes sense of its modalities and which shows how an hypothesis is changed as it is tested in practice and experiment.

The account would, of course, require that we use the notion of proposition differently, that we give up describing inquiry as a process of moving from considering a proposition to believing or disbelieving it, as if inquiry were only a change in attitude and attitudes are outside of propositions, are directed toward them, but never able to penetrate inside. The notion of a proposition is appropriate and useful when we want to talk about formal relations and the meaning of one thing's being a reason for believing another, not when we want to describe a course of reasoning. For the description, a notion of belief is better—because beliefs are not crisply singular and countable. In inquiry, they merge together, becoming stronger or secure, or strands in them are loosened and conviction is then strained, weakened, or lost. Inquiry is not a process of moving from one sharply outlined item to another or of having attitudes about such items change. It is an incorporating of items to have them more surely believed or a separating of an item so that, apart from a body of beliefs, conviction does not spread through it. Professor Quine has talked about beliefs we would be reluctant to give up because of their role in a body of beliefs. The complementary notion is what Peirce is suggesting: that beliefs can be brought into community, that they tend to lose their singularity when they are (though they can be isolated again), that this loss of singularity is a change in their meaning and their modality, and that it is brought about in a course of testing and experiment. Again, if we talk first of propositions, we cannot say that practice affects the work of thought. If we think of beliefs as the substance and life of inquiry, following Peirce, we can see how they are affected and somewhat changed.

II

The essays in the second group help us to understand Peirce by comparing his thought with other philosophical views. Many essays in the volume are of this sort, and they are helpful or not depending on our understanding of the philosophers with whom Peirce is compared. Richard Bernstein's essay, for example, is likely to be of special use; it is a careful contrast of Peirce's theory of perception and the theories of Descartes and Russell. Other essays are about Peirce and Scotus, Darwin, and James, and there is a particularly fine essay by Victor Lowe in which Peirce and Whitehead are compared. The most intriguing essay in the group is Krolikowski's on 'The Peircean Vir'. It is about Peirce's view of the connection between clearness and moral conduct. Unfortunately, it is framed as a comparison between Peirce and the elder James and Swedenborg, and since few of us today are informed about their thought, Krolikowski's fine article is not likely to receive the wide appreciation it deserves. The variety of the comparisons made by the essays in this group and the differences in their quality are so great, though, that I can think of no ready way to say anything useful about them. I'll go directly to the essays in the third group.

III

These are varied too, but many of them are especially critical of Peirce's categories of quality and law. These are important notions for Peirce and, on certain points, his critics are, I think, acute and sound. Let me, then, confine the remainder of this review to the criticisms of these two categories. The issues are about the connection between particular cases and a general rule, the short run and the long, and about what Peirce says in the reality of law and generality. They are dealt with in essays about Peirce's theories of probability and his account of evolution and cosmology.

Everyone who writes about induction and probability says that Peirce made important contribution to their analyses. Still (almost) no one is satisfied that Peirce's views are right. Peirce is sound, his critics say, in not construing an induction as a deduction in which there are suppressed premisses about uniformities or limits in the variety of nature. It is also sound to talk about approximating the limits of a series and the frequencies with which the conclusions of certain kinds of arguments are true. Still, though all this is sound, Peirce did not have it exactly right because (critics, again) he could not show that a short run provides a reason or a good reason for concluding what a long run will turn out to be. Peirce's justification of induction therefore fails, and probabilities are no guide to belief or action in any single case. These familiar complaints are made in several of the essays. They are made most emphatically by Professors Madden and Burks. Can Peirce answer them?

Professor William Lenz has also written about induction and probability, brilliantly, I think, and his essay shows the sense and deep strength of Peirce's view. Lenz does not argue that Peirce was right and that his critics are obviously wrong, but that Peirce built his view on suppositions his critics overlook, so that his fullest arguments have not been opposed.

Peirce claims that induction is self-corrective. Lenz explains that this is analytically true, for if we keep in mind how he understood 'reality' and 'true belief', 'we can see that . . . Peirce . . . is defining "truth" and "reality"—however crudely—in terms of limits of relative frequencies' (p. 154). This explains why he could claim 'that the rule of induction will hold good in the long run may be deduced from the principle that reality is only that object of the final opinion to which sufficient investigation would lead' (2.693, quoted on p. 155). That the real shall be cognizable, then, according to Lenz, is the basis for Peirce's strongest claims about induction's self-corrective character. The principle is that 'what cannot be known . . . simply cannot be'; and 'far from being a sheer assumption, it follows from Peirce's metaphysical view of reality' (p. 156). If the real must be knowable, there can be no series in nature without a limit to a given relative frequency. If there were 'we could not know . . . what the limit was, but neither could we know *that there was no limit*' (p. 157). This, of course, opposes those criticisms, like Madden's and Burk's, which turn on the idea that a series could have a limit which we might in principle never know.

Peirce's view is important in its own right and important, too, because it opposes that play with possibilities which makes for so much factitiousness in discussions of induction. We are told too often that no matter what has been found out about a class or series of events it is always possible that further cases will be quite different. Then, if we ask how, were this true, we could have a reason for believing something about other members or cases, or what it means to have a reason for such belief, we are advised not to look for deductive connections (as if that had to be what we were looking for) and we are reminded that there really are cases in which we say we have a good reason (which is of course true but beside the point). Why suppose, though, that there are these wonderfully vagrant possibilities? Why is it always possible for one of them to come about in fact? Even if something is possible because we think of it, thinking does not make such a possibility a possibility-for-fact. The agencies by which possibilities are brought about cannot themselves be merely possible. Peirce's view, as Lenz reminds us, is that what can possibly occur is connected now and directly with the entities and agencies that will cause it to come about. It is real and detectable; it would not be a possibility-for-fact if it were not. It is only when we separate the possible from the agencies by which it will be brought about that we are free to think that there are any possibilities we like; and then, of course, neither past experience nor contemplation can reveal which of them will occur in fact. Gratuitous possibilities make 'the problem of induction'. We do not have *that* problem if, like Peirce, we think of the possible as real.

We have other problems, though, for example, about the connections between what is possible and the regularities of agency by which things come about. Peirce calls operative regularity 'thirdness', and two questions his critics raise with him are why he claims and how he shows that regularity is a distinct categorial feature in what is real. Manley Thompson, for instance, argues that Peirce's experimental proof about the reality of regularity is rhetorical or circular and that Peirce never settled how he could support his categorial themes. There are other questions too, about what possibility and law explain, whether Peirce has characterized them properly, or whether his notions are beyond all saving too obscure. These are raised most forcefully in the studies by Professors Oliver, Hartshorne and Wells. Together, their studies confront Peirce with a dilemma: either the categorial notions cannot describe or explain what Peirce thought they could, or if they do, they do it either inaccurately (because Peirce misconstrued them) or far too loosely (because he has run together notions which he claims are distinct, so that his descriptions are hopelessly vague).

Let me set out the dilemma more specifically: Peirce says that the lawfulness which is exhibited in occurrences comes about either *from* something which is possible or *in* something which contains a possibility of having a law-like character. The dilemma turns on the alternative. Are the pregnant possibilities in occurrences or not? Oliver thinks that for Peirce to make good what he says about a possibility's being what it is in its own right, he has to consider possibility apart from any change or temporality; but then he concludes that Peirce fails 'to account for *this* world as one of a limited class of possible worlds' (p. 297): that Peirce's notion cannot explain what he thought it could. Hartshorne and Wells, on the other hand, seem to think that, for Peirce, possibility has to be caught up in occurrences and that, therefore, Peirce misdescribed it by saying that it is non-relative; or, what is worse, he cannot distinguish it from law or regularity.

Given the alternative, the dilemma is inescapable. If possibilities are discrete and independent entities, there also has to be an agency or power which explains their pertinence and presence to or in ocurrences, which is another way of phrasing Oliver's complaint. If possibilities are not independent of the agencies and powers which affect their becoming present, then, as Hartshorne says, they are at least partly relative and therefore not properly categorial, or they are finally indistinguishable from the distinctiveness of operative laws: one of Wells' points. Peirce has set out his conceptions of possibility and law so that there is either no connection or no difference between them.

These are difficult issues, of course, but Oliver, Hartshorne, and Wells have not misconstrued Peirce's view. There is something wrong with Peirce's categories of possibility and law and these critics bring out what it is. The mildest corrective I can think of is to admit the force of the dilemma, but then to argue that it is sound and inescapable only if there are many possibilities, all discrete. Were we to claim instead that possibility itself is categorial, that there is a generic possibility which comes to be specialised into real possibilities, we could claim most of what both Peirce and his critics see. Though modest, this corrective would require a change in each of Peirce's other categories. We would have a new scheme, but one, I think, which would be close to the ideas Peirce was working on towards the close of his life.

IV

The preface to this book says that its studies show us 'the situation in our understanding' and appreciation 'of Peirce today' (p. v). What does the situation seem to be?

We see, I think, that we are almost done with ceremonial appreciations of Peirce. We are not likely to have more of those essays in which Peirce is celebrated as the greatest of American philosophers and great in any company. Few of them had much incisiveness anyway, and such stature, pride and posture as they give to American philosophy does not seem needed now. There are now fewer provincialisms in American philosophy than there were ten or twenty years ago. Where Peirce used to be lionised and then protected, few of us now seem especially defensive about the confusions in his thought. Readily, most are willing to say that Peirce often wrote badly and that the strands of his thought do not fit together into the proportioned system he proposed to make. The day of the two Peirces, one hard headed and one soft, are gone, or almost gone. We are done with idealizing Peirce and almost done with the outlining of his thought.

Has there been or is there now a strong influence from Peirce in the making of new thought? My impression is that recently there has been little influence, or rather, that there is not much right now; if the present volume is indicative, there is likely to be even less in the years ahead. Many of us have seen, of course, that Peirce anticipated a good deal in contemporary thought. But while seeing this, 'the situation today' seems to be that we are inappreciatively critical of Peirce. The most appreciative of the critical studies in the volume are about Peirce's logic but, as I tried to show, they overlook Peirce's main concerns. In fact, there is hardly an essay in the volume which suggests where or how we can still learn from Peirce, or what lines of thought might usefully be followed up. This is in noticeable contrast to the first volume of studies, published in 1952. In that book, many of the essays used Peirce's thought as a start in making a contribution to, say, an account of inference, the categories and the good. In the present volume, there is no excitement toward constructiveness. The essays are about Peirce; they explain him, are critical of him, but generally the essayists appear to think that they have learned what they can from Peirce and that they will now turn to other thinkers to provoke and test their own further thought.

What the volume suggests, then, is that we understand a fair amount about Peirce but that Peirce is not likely to be one of the thinkers against whom or out of whom the leading thought of the remainder of this century will be made. Morris Cohen said in his preface to the first collection of articles by Peirce that Peirce was a seminal thinker. Cohen was right, only the growth of thought that comes from Peirce nowadays seems not to be very wide or deep or to have much force. There would unfortunately be little effect in arguing that this ought not to be the case.

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Richard Foster Jones: *Ancients and Moderns. A Study of the Rise of the Scientific Movement in Seventeenth Century England*, 2nd edition (University of California Press, 1965. $1.95). Pp. xiv+354.

At a time when historians of science are increasingly conscious of the wider cultural setting of the 'Scientific Revolution', the reappearance (in paperback) of Professor R. F. Jones' pioneering study, *Ancients and Moderns*, is to be welcomed. Conceived as a study of the background to the 'Battle of Books', which left a literary memorial in Jonathan Swift's work of the same name, it showed that the roots of the battle reached as far back as the controversy over the 'decay of nature' which was already active in the early part of the century.

But it accomplished much more than that. In investigating the dominance of scientific claims in the English *querelle*, it studied the English scientific movement in a much broader context than was usual then, or is even now. The task was performed by examining an immense mass of materials, and although some of the issues raised then have been since studied in greater detail (sometimes in ignorance of Professor Jones' earlier work), the ground covered and the range of authors and evidence cited in the notes makes the work a notable *tour de force.* The 'decay of nature' debate, the influence of Gilbert and Bacon, the propagation of 'Baconianism' by the university reformers of the Commonwealth period, the Royal Society and its critics: these are only some of the themes treated in detail by Professor Jones. He was among the earliest to appreciate the importance of Paracelsian and Helmontian currents and the way in which their proponents could join hands with 'Baconians' in urging a new approach to the study of nature, a theme which has been explored in greater depth in recent years.

If the work retains its value as a bibliographical source and a suggestive guide to authors and issues that still await greater attention, the three decades which have elapsed since its first publication make it possible to discern some of the limitations and structural weaknesses in its approach. These spring in part from the literary milieu in which it was first conceived. In the 1920s and 1930s, the attention of students of English literature was forcefully drawn to the impact of the 'New Science' by T. S. Eliot's concept of a 'dissociation of sensibility' which he traced through the seventeenth century between Donne and Dryden.[1] The forces which had brought about the 'dissociation' were soon identified by a number of authors with the 'New Science'. Professor Jones was himself very consciously tracing the roots of the 'materialistic', vocational and technical idea of education, which seemed to him the greatest threat to the humanities, especially in the United States, at that time. In his own words, 'our modern scientific utilitarianism is the offspring of Bacon begot upon Puritanism' (91).

A great many oversimplifications flow from these assumptions. The basic thesis is that the ideas of Bacon, ignored for the most part during his own lifetime, were seized upon by the Puritans during the 1640-60 period. The *experimenta fructifera* began then to eclipse the *experimenta lucifera.* The Renaissance magnificence of the *New Atlantis* was reduced to Hartlib's prosaic *Macaria.* Certainly, the relatively sudden popularity of Bacon's ideas during the troubled decades, and the rôle of men like Hartlib and Dury in propagating them were features that required emphasis; but the simple identification of 'Puritans' and 'Baconians' leads to serious distortions. Hartlib and Dury were 'Baconians', as were the would-be university reformers Noah Biggs and John Webster. Therefore there is little to distinguish them from the 'Baconians' of the London and Oxford groups of the period. That is made explicit in the comment on Webster's *Academiarum Examen* (1654): 'Webster's treatise is the most important expression of the new scientific outlook between Bacon and the Restoration' (108).

It is an astonishing judgment. A closer examination of Webster's treatise must make it clear that although he cites and praises Bacon, Gassendi, Galileo, and Descartes, the natural philosophy he wished to teach in place of discredited Aristotelianism was of a Paracelsian and Helmontian cast. Indeed, he concludes that the most perfect piece, expounding natural philosophy on 'sensible, rational, experimental and Scripture principles' was that of Doctor Robert Fludd.

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[1] *The Metaphysical Poets* (1921).

Similarly, Comenius' *Natural Philosophie Reformed by Divine Light* (published in English translation in 1651) claimed to derive the basic scientific principles from the text of *Genesis*. Sectarian authors whom Jones sees as disciples of Bacon were, in fact, presenting a particular interpretation of Bacon, which departed most markedly from the spirit of his own work not only in its crude utilitarianism, but in its involvement of Scriptural texts in natural-philosophical controversies, in the stress on grace and illumination, and above all in the use of a certain Neo-Platonic schema of explanation Bacon had sought to reject.

If we revise Professor Jones' thesis in this manner, we must consider Ward and Wilkins' polemic against Webster (in their *Vindiciae Academiarum* of 1653) a serious attempt to differentiate their 'Baconianism' (and, in so far, as they were its mouthpiece, that of the Oxford group) from the enchanted versions of the sectarians. One way of distinguishing between the two approaches would be in terms of their attitude to the 'mechanical philosophy'. In contrast to the attempts since the sixteenth century to refurbish scholastic natural philosophy or to provide various Neo-Platonic alternatives to it, the 'Mechanical Hypothesis' furnished a radically novel system, comparable to the Aristotelian one in range and coherence. The English versions of that philosophy are to be found in such different authors as Sir Kenelm Digby, Thomas Hobbes, Walter Charleton, and Robert Boyle, who were reacting primarily to the stimulus of continental thinkers like Galileo, Gassendi, and Descartes. Although aware of the importance of the 'Mechanical Philosophy', Professor Jones does not use it as the dividing line. In order to claim Bacon as the father of the 'New Philosophy' in England, he is led to introduce a highly questionable distinction between an 'experimental philosophy', deriving from Bacon, and a 'mechanical philosophy', from Descartes. The Gresham and Oxford groups and the early Royal Society—indeed, the English scientific movement in general—is viewed as owing allegiance to the former, and therefore to the commanding figure of Bacon.

No doubt, it is possible to make such a distinction plausible, and even to strengthen it by additional considerations not included in the study. Some of the leading members of the Gresham and Oxford groups (significantly enough, the physicians like Ent, Goddard, Willis, and Glisson) cannot be aligned with the mechanical philosophy in any simple fashion; Willis and Glisson in particular explicitly rejected it. Even in regard to those who basically accepted it (the younger members like Boyle, Wren, Hooke, and Wallis), it is necessary to be far more sensitive to the limitations which they placed on its range of application than is usual in current discussions. Nevertheless, it is possible to make too much of the hesitations of Boyle, and to take the often proclaimed 'freedom from systems' of the apologists of the Royal Society far too seriously. There can be little doubt that, as on the continent, so in England the brilliant achievements in the mechanical-mathematical sciences of the later seventeenth century were rooted in the 'mechanical philosophy', and laid the foundations of a modern science which marked it off from the earlier natural philosophy.

Another basic weakness is the failure to define with any degree of clarity what is to be understood by 'science', and particularly by the 'New Science' whose emergence is held to be distinctive characteristic of the seventeenth century and provides the occasion for the study itself. One gathers that it is to be associated with Francis Bacon above all, and that it is connected with the study of natural phenomena by observation and 'experiment', postponing the building of systems until several generations of investigators have built up a sufficient stock of data. Given this definition of 'New Science', it is not surprising that every author who praised observation and 'experiment' is identified with it. Hence, Hartlib and Dury, Biggs and Webster, Boyle and Wallis, are Baconians all and modern scientists. That the term 'experiment' then signified a general appeal to experience (not excluding mystical experience), and that its meaning varied and was anchored in very different presuppositions even as between, say, Bacon, Gilbert, and the Oxford group is completely lost from view. The deficiency is not peculiar to Professor Jones' work, and will be found in numerous other studies written during the same period, and especially in those which, like Professor Jones, claim a link between Puritanism and science. It is important that this feature of the interpretations advanced by these authors should be recognised, for it then seems easier to establish an elective affinity between the variants of the Neo-Platonic magical philosophies of nature of the Renaissance (of which the Paracelsian is an outstanding example) and sectarian Puritanism, rather than 'New Science' and Puritanism as such.

The work also suffers from a deficiency that has characterized much Anglo-American work on the history of ideas: the choice of England as a self-contained unit of analysis. Hence, a host of errors and over-simplifications, and a gross over-estimation of the originality of various English thinkers. In Jones' work, for example, there is too great a readiness to postulate a 'Gilbert tradition' and to assign authors to it by reference to various general attitudes which were, in fact, commonplaces of European intellectual discourse at the time. (It is assumed, for example, that John Johnston's treatise (1632) must have been derived in the main from Hakewill (1627) because of a broad similarity of treatment, although Johnston had spent most of his life abroad.) There is evident throughout an ignorance of the wider European setting, which would have clarified the treatment of such currents as libertinism, scepticism, and natural magic (the definitions of natural magic which were given classical form in the works of Florentine Platonists are quoted, for example, as if they originated in the English authors from whom they are cited). There is very little on continental influences beyond a few dubious conjectures about the influence of Giordano Bruno (through his English visit in 1583). Since a 'Gilbert Tradition' is suggested in the book, it is appropriate to point out that a careful reading of the *De Magnete* and of the contemporary theories of magnetism discussed in it provides ample proof that Gilbert saw himself as part of a European world of learning, to which, indeed, his work was addressed. Continental influences made themselves felt not only through the works of continental thinkers. Some of the leading English natural philosophers had benefited greatly from European visits, whether voluntarily (for example, Harvey, Hobbes, Browne, Boyle, and Petty) or in exile (Cavendish, Digby, Thomas White). If the history of the English scientific movement is to be studied as part of the intellectual history of the period, the task can satisfactorily be accomplished only in the context of a European republic of letters.

Since Professor Jones was among the earliest to stress the connection between Puritanism and science in seventeenth-century England, it appears somewhat ironical that, while overstressing the influence of one particular religious current on natural philosophy, he has, at the same time, vastly underestimated the full impact of religious influence on natural philosophy and exaggerated the secularisation and 'materialism' of the English thought of the time. Thus, in an otherwise incisive account of Francis Bacon, there is no hint of the profoundly religious conception which provided the basis for Bacon's programme, the dream of using a new science of nature to repair the effects of the Fall of Man. Nor is there any appreciation of the fundamentally religious—even apocalyptic—setting of the 'idea of progress' in England till the end of the century. It is not too much to suggest that neither Bacon nor Newton viewed progress in the secular framework that is sketched in this work.

Finally, some doubts must be raised about the usefulness of organising a study of the English scientific movement around the controversy over ancients and moderns. It seems very doubtful that the issue was one of central importance during much of the century. The ancients-moderns antithesis is conspicuously absent from some of the most significant works of philosophy and natural philosophy—those of Herbert of Cherbury, William Harvey, Gilbert, Digby, Charleton or White. Only rarely is it more than a subsidiary theme. More often than not, the debate was conducted in terms *not* of moderns against ancients, but of certain ancient thinkers against another ancient, the Aristotle of the schools. It is unhistorical to see here no more than a clever tactical device enabling 'moderns' to challenge orthodoxy. Increasingly through the sixteenth century, classical scholarship made the knowledge of ancient alternatives to Aristotelianism available. These ranged from the pseudo-Hermetic works, to Archimedes, Heron, and (through Lucretius and various other sources) Democritus and Epicurus. While the discrediting of ancient authority through criticism of the newly purified works of Ptolemy and Galen is familiar, it is easy to overlook their displacement by other ancients, whose doctrines and systems won favour and suggested or legitimated different ways of studying and explaining natural phenomena.

The fate of atomic doctrines in the sixteenth century is instructive in this regard. In dealing with *mixtion* problems as in explaining away the 'prodigies' of recrudescent and now intellectually respectable Natural Magic, purer Aristotelians like Fracastoro were being driven towards atomic-type explanations. By the end of the century, the religious respectability of atomic doctrines was established by the Mochus-Moschus account of its origins, which was resurrected from Strabo and Sextus Empiricus by a late sixteenth century philologist. They now appeared part of a Phoenician natural philosophy, and possibly even Mosaic in origin. Pythagoras had brought them to the Greeks, among whom they had been corrupted and 'atheized' by the Democriteans. It was an enormously influential account, and such atomists as Sennert, Basso, Magnenus, and Claude de Bérigard saw themselves as revivers of an ancient current of thought. Just as Magnenus represented himself as a reviver of Democritus, so did Maignan of Empedocles, and Gassendi of Epicurus. In England, Henry More and Ralph Cudworth elaborated the Mochus-Moschus account to fit the 'Mechanical Philosophy', and both Boyle and Newton were clearly influenced by it. In a view of history in which the 'moderns' appear as no more than recoverers of a lost ancient wisdom (the successful recovery of true knowledge itself interpreted as heralding perhaps the end of the Christian drama), the idea of progress bears a very different significance from that which it has been given in Jones' secularised version.

To re-read this work is to realize how much more complex the period now appears to us. While encouraging current gropings towards a 'history of science' taking much greater account of the broad cultural framework of a period, it shows that such a treatment can greatly oversimplify if key terms like 'science' and 'scientist' are defined without reference either to detailed natural investigations or to conceptual debates and transformations. A particular 'inductive' conception of science, influential among many philosophers of science of the time, is implicitly adopted and distorts the interpretation throughout the work. The lack of a comparative perspective and a serious under-estimation of religious influences are among other major defects. If a re-evaluation of the work helps to clarify our thoughts about the ideal history of science envisioned by Comte, it is a salutary reminder of the varied skills which will be required from a practitioner of the craft.

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F. Brentano: *The True and the Evident* (London, Routledge and Kegan Paul, 1966, 40s.) Pp. xxix+190.

The present book is an English version of *Wahrheit und Evidenz*, which was published in 1930. It was a collection consisting of a lecture, essays, previously published notes, unpublished fragments, and letters by Brentano. These were all on the subject of truth and knowledge, and were chosen, introduced and annotated by Oskar Kraus, who had been authorised by Brentano to edit his remains. In the translation a two-page Preface is added by Professor Chisholm, the present editor; eighteen pages of the book represent Kraus's 1930 Introduction, and forty-two his Notes. The English version of Brentano's own text occupies 129 pages.

Though fairly free, the translation (which I have checked only in places) seems reliable enough in general. Occasional sentences are doubtful, while the second sentence of the Husserl quotation high on page xxii is certainly mistranslated. And should not 'value' on page 15, two lines from the last, be 'belief'? On page 58, for 'note 73' read 'page 73' (=note 69, page 99 of Hague's English version of *Vom Ursprung*). On page 65, surely 'main clause' would be a clearer rendering than 'primary sentence'? Despite what is said in para. 3 of the Preface, 'real' and 'reality' are used on page 73 in the way resolved against. The words 'der Glauben sei hier instinktiv und mechanisch' on page 61 of the original are not represented by anything on page 52 of the translation; one hopes this is not a common occurrence. The index of the present book is in general better than the one in the original, but has omitted certain interesting items altogether. These should be replaced as follows (page numbers are adjusted): autosemantic, 162/intuition, 25 90, 92, 108, 175 [the German word is 'Anschauung', and would be more intelligibly rendered in all the passages as 'perception']/Jerusalem, W., 182/Meinong, A., xvi, xviii, 149/Stumpf, C., 149/Wittgenstein, L., 163; furthermore, 'Grotius, Hugo' should read 'Grote, G.'

Incidentally, editors seem not to check for nonsense provided it is in Latin. In the quotation from Descartes on page 29, the first word, 'ordines', has been passed by Chisholm, as it was by Kraus in 1930, by Hague in 1902, and possibly even by Brentano himself in 1889. G. E. Moore must have seen it made no sense, as he marked it with a query in his copy of Hague's translation (now in the University Library, Dundee). One should read 'omnes'.

The passages of Brentano contained in this book illustrate the development of his views on truth and knowledge from 1889 to 1915 (he died in 1917). His gradual abandonment of the correspondence theory of truth (part of his strongly Aristotelian beginnings) was accompanied by an insistence that the objects of judgment must be *realia*, genuine objects, i.e. things, though they need not be *existent* things. Thus Brentano's later view does away with *entia rationis* of all kinds, e.g. besides contents of judgments, states of affairs, and subsistent eternal truths it dispenses with 'an infinity of impossibilities, the beings of these impossibilities, and the non-beings of the beings of these impossibilities, and the non-beings of the non-beings of other impossibilities, as well as of the non-beings of *realia*, or *things*' (86). Indeed, the teacher of Meinong and Husserl is already to be found brandishing Ockham's razor in 1901, writing to Marty (64) 'All abstracta are now to be counted as delusions' (*alle Abstrakta kommen in die Klasse der Wahngebilde*).

The present-day reader is more likely to be attracted (and equally, surprised) by this movement towards a minimum ontology than by the other strand in Brentano's thinking, the definition of truth in terms of the evident (*Evidenz*). The evident seems to be the same as the necessarily true, in that it has nothing to do with obviousness, being objective (110) and sometimes discoverable indirectly ('if a judgment is evident, then either it is directly evident or it is evident as a result of a proof connecting it with other judgments which are directly evident'—120). In Cartesian fashion, Brentano includes knowledge of the self, when restricted to the present, in the evident. What is disturbing about the evidence-theory of truth is not the thinness of the accounts of the evident—where in other authors is there a good account of necessary truth?—but the failure to take empirical truth seriously. Once the correspondence theory is abandoned, what Brentano calls 'a blind judgment which happens to be true' can have its truth determined 'only to the extent that we can compare it with a judgment which *is* logically justified' (102). What Brentano requires for a 'blind' judgment to be true is that it could not be contradicted by an evident judgment. This is quite insufficient as a criterion to distinguish blind judgments which happen to be true from those that happen to be false. What is called by Kraus on page xxiii, 'the final overthrow of the correspondence theory' is not merely incomplete here; it has not even begun.

To return to Brentano's later ontology. This, for all its attractiveness, is perhaps no more adequate philosophically than his theory of truth. For it rests on the denial that anything other than things can be the objects of thought: 'only *things* can be thought' (67); 'only *things* can be objects of our ideas and therefore . . . only things can be objects of any type of mental activity at all' (94); 'only things can be objects of thought and of affirmation' (114). And yet it remains unclear what a thing in these contexts is supposed to be. Thus on page 67 Brentano says 'I have in mind whatever is such that, if it exists, it is a substance, an accident, or a collective of both'; elsewhere he says, 'everything is a thing or entity' (72); and 'I would say that relations and concepts such as ***shape, extension, position*** (I am speaking of the concreta in question) are included among things' (80). The upshot is problematic. As he puts it himself on page 86: 'we have complications enough if we accept things as our sole objects'.

Brentano was extremely influential in his time. Besides Meinong and Husserl, Freud, Ehrenfels, and T. G. Masaryk were numbered among his pupils. But he is a philosopher's philosopher, caviar to the general, very much like G. E. Moore, who reviewed very favourably the only book of his that has previously appeared in English (*The Origin of the Knowledge of Right and Wrong*, transl. C. Hague, 1902, pp. 47-69 and 71-78 of which correspond to pp. 28-59 of the present book). These were two men of comparable stature. Whether Brentano was 'the foremost philosophical mind of our age', that is of the early twentieth century, as Kraus claims on page xxiii, is highly disputable. However, he is clearly of permanent interest.

Therefore in producing this translation Professor Chisholm has done a notable service to the philosophic world. In effect he has rescued Brentano's ontological views from undeserved oblivion, since practically no philosophers whose native language is English ever have, or would have, read *Wahrheit und Evidenz* in the original.

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J. D. North: *The Measure of the Universe* (Oxford, 1965, 70*s*.) Pp. xxviii+436.

The first two-thirds of this book consists of an account of the origins and development of cosmological theory up to about 1950. The author prepares his ground by describing the gradual growth of understanding of the nature of galaxies and nebulae, early attempts to apply Newtonian theory to cosmology, and early generalisations of Newton's theory of gravitation. A discussion of Einstein's theory of gravitation is followed by an account of the development of cosmological theory based on general relativity, of the gradual disciplining of theory by observations, and of the various later cosmological theories.

The last third of the book discusses philosophical issues raised by, and illustrated by, the development of cosmological theory. The author devotes chapters to the concept of the Universe, the role of convention in assigning structure to space-time, the role of idealisation in formulating cosmological principles, and various conceptual problems arising in cosmology. The two parts are interlinked in that the first part concentrates on aspects of the history with philosophical interest, and the second part gives the historical development of the issues discussed.

While the author's description in the first part is in many ways very complete, it assumes familiarity with Newton's and Einstein's theories of gravitation. Thus the equations of Newton's theory are not written down; the transition from a definite to an indefinite metric is made without any discussion of physical interpretation; the phrase 'null geodesic' is introduced without explaining either word; neither the Christoffel symbols nor the Riemann-Christoffel tensor are defined. The reviewer feels this is a pity; the account could have been made self-contained in this respect, at the cost of only a small increase in length.

Development of cosmology has been rather rapid since 1950. Clearly the author could not aim to give the whole history of this period in his book, but omission of some of these later developments results in obscurities which could have been avoided. For example, the discussion of the principles of Equivalence, Covariance, and Relativity is unclear by recent standards; in particular, one must question the author's claim that the principle of covariance emphasises the topological aspects of physical laws. The terms 'stationary' and 'static' are now used to describe precise geometrical situations (the author uses 'static' to describe what is now called 'stationary', and leaves 'stationary' indeterminate). The author criticises the use of the inverse Hubble constant as an estimate of the age of the Universe; however, using Robertson's energy conditions, one can prove that the inverse Hubble constant is an upper limit on the age of the Universe in a wide variety of Universe models. The nature of the 'singularity' at $r = 2m$ in the Schwarzschild solution is now well understood (the author's account of the Schwarzschild solution is not clear, but contains references to papers in which correct discussions are given). It is incorrectly stated that in General Relativity the velocity of light depends on the distribution of matter.

Despite these qualifications the comprehensiveness of this detailed historical account will earn *The Measure of the Universe* a place on the shelves of anyone interested in the history of modern cosmology.

The author's philosophical discussion is interesting, but is sometimes woolly. For example, a statement is said to be 'morally certain', and a qualitative argument is disguised (on p. 311) as a mathematical argument. The author's discussion of 'fact' in modern cosmology is rather unsatisfactory. Cosmological principles are separated into 'laws', 'meta-laws' and 'high-level hypotheses' without any clarification of these terms; this discussion could be made clearer by making a distinction between physical laws, on the one hand, and boundary conditions on the other hand. The 'distance' between two events in space-time may be defined in a number of different ways, depending on the method of measurement envisaged. The author spends some time justifying the rather arbitrary decision to call one of the distances 'more fundamental' than the others, yet underestimates the importance of the ascription of *some* coordinates to events in space-time. The discussion of various concepts of 'space' would be made clearer by introducing the various mathematical concepts of spaces (topological spaces, affine spaces, etc.).

Some of the points raised deserve further discussion; a particular example is the concept of absolute time. The author is incorrect in stating 'it should not be thought that [Gödel] found any inconsistency in the idea of a cosmic time'. A cosmic time coordinate in the usual sense is *incompatible* with the motion of the matter in Gödel's universe, since one cannot choose a coordinate t such that the spaces, $t =$ constant, are the local rest spaces of the galactic observers. Even if one ignores this requirement there still exists no satisfactory time coordinate in this space-time, since *whatever* time coordinate one chooses, there exist observers (not galactic observers) for whom this time *decreases* as their own proper time increases. The author dismisses the causal difficulties of Gödel's universe (it is in principle possible to travel into the past and the future, and influence either) by stating 'this property must be judged an absurdity by anyone committed to the ordinary modes of speech'. This response is inadequate, as Gödel's solution proves that the existence of a closed (personal) time is compatible with general relativity. If this were to be the case in the actual universe, our usual notions of causality (and normal modes of speech) would be substantially incorrect. The concept of a closed personal time only gives rise to *logical* difficulties if the usual concept of casuality is correct.

One would also like to see a clear discussion of the problems raised by the existence in cosmological models of regions of space-time which cannot *ever* be observed by us, whatever direct or indirect means of observation we use. The scientific status of such regions is suspect; one can say what one likes about them without fear of contradiction. The same problem is raised in a weaker form, by the existence of regions of space-time about which we will have no information whatever within, say, the next 1,000 years. If these questions are taken seriously, a radical reappraisal of the concept of a cosmological model must result.

While this part of the book does not provide a definitive discussion of the philosophical questions raised by modern cosmology, it does provide a stimulating and useful account of them.

G. F. R. ELLIS
Peterhouse
Cambridge

Ian G. Barbour: *Issues in Science and Religion* (London, S.C.M. Press, 1966, 45*s*.). Pp. x+470.

This is an astonishingly surefooted book. It explores an area in which traps abound for the half-competent, and books abound which have failed to avoid them; and it not only picks its way triumphantly past practically all of these, but also succeeds in turning a penetrating light on some of the most troublesome as it hurries us by. Designed for use as a course textbook, it is compact and didactic in style rather than popular or polemical, and each chapter ends with a stocktaking summary which is a masterpiece of clarity. The author's personal view is not concealed, but the alternatives are expounded with such fairness that one often has to look ahead to discover which he will eventually favour, and at first glance the treatment may sometimes even seem uncritical. In fact, however, the reader who stays the course will emerge with a coherent and well-articulated critique of the main options, and a clearly-argued case for the Whiteheadean 'critical realism' in terms of which Dr Barbour himself finds science and Christian religion to be best integrated. As both a Professor of Physics and Chairman of a Department of Theology, he should be well-placed to put his view to the test.

Inevitably, much of the book is devoted to ground-clearing, aided by plentiful quotations from original sources. Reductionist arguments against religion are shown to be philosophically baseless, and the distinction between the biological theory of evolution and metaphyiscal 'evolutionism', still apparently much-needed, is particularly well brought out. There are sensible and stimulating comments on such topics as the nature and goals of scientific inquiry and explanation, the uses of theory, the extent to which orderliness in nature is presupposed in science, and (*contra* Collingwood) the importance of testable interpretive concepts even in the 'empathic' processes of the historian.

The same common sense illuminates the second half of the book in which religion and contemporary science are brought into confrontation. Dr Barbour will have none of the escape routes whereby religious statements are reduced to declarations of policy or the like. Cognitive assent, he maintains, is an essential aspect of the Christian religion. He sees equally clearly the limitations of the approach of natural theology. 'There are moral as well as intellectual conditions for knowledge of God . . . (as) even for the knowledge of oneself or one's neighbour' (p. 222). As one of the most important epistemological points made in the book, one would have liked to see this argument developed at greater length. On the vexed question of 'commitment', he finds 'religion and science to differ in the degree and form of involvement, not in any absolute contrast' (p. 228).

What then of the relation between the two? 'Let it be granted that the scientist is interested in nature as a lawful structure, whereas the theologian is interested in nature as related to God and to man's life-orientation . . . (Still) it is . . . the same natural world to which they look, so their inquiries cannot be totally independent'. A consistent set of metaphysical categories must be sought in 'dialogue between two communities that respect one another's integrity' (p. 270).

This dialogue is astringent. 'It would be as dubious to attempt to build a metaphysics of idealism on modern physics as it was to build a metaphysics of materialism on classical physics. . . . The attempt to found a concept of human freedom on atomic indeterminacy is as guilty of reductionism as was the earlier denial of freedom on the basis of classical physics' (p. 315). The Heisenberg Principle is indeed taken to indicate 'objective indeterminacy in nature rather than the subjective uncertainty of human ignorance', and 'alternative potentialities' are said to exist for individual events; but the temptation to restrict God's action to the domain of physical 'chance', as Pollard seems to do in 'Chance and Providence', is resisted.

Instead, the author argues on the lines of process philosophy, for a recognition of 'novelty and freedom as well as lawfulness' at all levels of reality, with God's action occurring 'perhaps primarily on the higher levels, which would in turn influence others "from the top down" rather than "from the bottom up".'

This is one of the few points in the book at which one feels that perhaps sharper formulation is called for. The sense in which the 'whole' at one level is truly 'more than the sum of its parts' at a lower level is never easy to articulate without seeming woolly or 'soft' from the lower-level standpoint. Dr Barbour's argument is excellent, but the effect is confused by a slightly ambivalent attitude towards the powers of automata, based apparently on the idea that cybernetic mechanisms can pursue only 'goal-objects' that actually exist. Where there are 'goal-ideas for which there are no goal-objects', such as 'the short route to India' sought by Columbus, the behaviour is said to be 'characteristic only of high levels of organisation in life and mind' (p. 341). On the other hand he seems to accept (later, on p. 352) that 'there is no reason to deny the use of mental concepts in the interpretation of the activity (of computers with sufficiently high levels of organisation). . . . Their parts are "inanimate"—but so are the "parts" of man.' Is the force of the earlier distinction then purely terminological? One feels that here some retrospective reshaping of the argument could considerably strengthen its impact on those who imagine that the only justification for a non-mechanical level of analysis (apart from personal predilection) lies in a proof that the behaviour in question could not be shown by a mechanism.

Certainly Dr Barbour's view of man as a 'many-levelled unity' requires no such justification, and seems to coincide most satisfactorily with all the biblical and scientific data to which justice must be done by a coherent theology of human nature.

A bibliography of some six hundred annotated references attests the thoroughness of this outstanding survey, which ranges over many topics unmentioned here. I know of nothing contemporary with a better claim to become a standard text.

D. M. MACKAY
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G. E. R. Lloyd: *Polarity and Analogy, Two types of Argument in Early Greek Thought.* (Cambridge University Press, 1966, 84*s*.) Pp. vi+503.

The author declares that his aims in writing this book have been 'to describe and analyse two main types of argument and methods of explanation as they are used in early Greek thought from the earliest times down to and including Aristotle, and to consider them, in particular, in relation to the larger problem of the development of logic and scientific method in this period'. The problem of development to which he refers is that of determining the stages by which men became aware of the need for making certain distinctions that now seem to us obvious, namely that between contrary opposites and contradictories and that between suggestive analogy and conclusive demonstration. Most of his book, therefore, is taken up with the marshalling of evidence in such a way as to illuminate the use of his two main concepts by various early Greek thinkers. His undertaking might indeed be said to be a re-writing of the history of early Greek philosophy from a point of view which specially interests him. In a central part of chapters which deal with analogy he surveys, for example, first the pre-philosophical background revealed by social anthropology and the study of the oldest surviving Greek literary texts, next the use of political, biological, and technological imagery in Greek cosmological theories, thirdly the role of comparisons in a number of particular attempts to explain natural phenomena (e.g. Empedocles' likening of the eye to a lantern), and finally a series of passages from Plato and Aristotle in which analogy is explicitly recognised and evaluated as a type of argument. Since the problem which inspires the enterprise is not a very *sharp* one, i.e. not a question to which there could be a very clear-cut answer, the general impression conveyed by the author's work is one of patient scholarship rather than lively argument, but I believe he is right in thinking that it may nevertheless be useful to philosophers and historians of science as well as to the classical scholars for whom it is primarily intended. When we talk and write in general terms of analogy as a source of hypotheses, we do well to consider the earliest as well as the latest examples of this pattern of thought.

In this connection I think Dr Lloyd might perhaps have done more to justify his linking of the two topics mentioned in his title if he had tried to show that the great importance assigned to polar opposition in early thought is due to excessive reliance on superficial analogies. Consider, for example, the Pythagorean table of opposites, which, as Dr Lloyd says, was probably the first attempt at systematisation of a very wide-spread way of thinking:

limit	unlimited
odd	even
one	many
right	left
male	female
resting	moving
straight	curved
light	darkness
good	evil
square	oblong.

Why should 'male' be on the same side as 'limit'? Presumably because the notion of limit suggested that of good order and males were thought (by males, who did all the recorded philosophising at this time) to be the better and more orderly sex. Dr Lloyd rightly remarks that there is no sound empirical base for such comparisons, but it is possible to go further and to say that the whole project of drawing up a table of related opposites rests on the mistaken assumption that all terms which are contrasted must be contrasted for similar reasons. In fact it is necessary to distinguish not only contraries from contradictories, as Aristotle succeeded in doing, but also scalar opposites (such as light and dark, hot and cold, dry and wet) from alternative varieties (such as odd and even, male and female, square and oblong) and again among the latter complementary but equally positive varieties (such as male and female) from paired kinds (such as odd and even) of which one is essentially negative. When once we have realised these and the many other distinctions that can be made among terms which have all alike been called opposites, we find it difficult in retrospect to see why our intellectual forebears thought it enlightening to ignore them for the purposes of theory-making. But this is only a special case of a difficulty which, as Waismann once said, we have in understanding all dead philosophy. What is interesting here from a methodological point of view is the influence of superficial language-based analogy in leading men to ignore in philosophising what they recognise quite well in the rest of their lives. For we must not suppose that the early Greeks had any serious difficulties in using contrasted terms correctly for non-philosophical purposes. The Homeric poems do not suggest, for example, that ordinary men ever supposed numbers to be more or less odd because actions could be more or less good, or women to be merely unmen because odd numbers are essentially uneven.

WILLIAM KNEALE
Oxford

W. H. Capitan and D. D. Merrill (ed.): *Metaphysics and Explanation* (Pittsburgh, The University Press, 1966, $4.50). Pp. 112.

The Annual Colloquium in Philosophy at Oberlin College, Ohio, has gained a high reputation for distinguished papers and penetrating discussion. This volume comprises the proceedings of the fifth Colloquium (1964) and amply justifies that reputation. It contains two essays and three symposia.

In the first essay, 'On Deductivism as a Philosophy of Science', Stephan Körner argues, against, he says, most philosophers of science, that the testable consequences of a scientific theory are not empirical propositions and that within a theory the reasoning from premisses to conclusions is not entirely deductive. This is because a scientific theory is an idealisation since it depends upon 'classical' logic and mathematics which can work only with exact concepts whereas many empirical concepts are inexact. Deductive reasoning within the theory, involving only exact concepts, consequently cannot yield empirical statements. We need also a non-deductive step whereby the deduced consequences are 'identified with' empirical propositions. If this is correct, there remains, of course, a problem concerning the meaning of 'identified with.'

J. J. C. Smart writes on 'Nonsense', arguing that the alleged two senses of this word, corresponding roughly to 'meaningless' and 'egregiously false' are more closely connected than is usually supposed. Whenever we are inclined to say that a sentence is meaningless it is open to us to adopt different 'rules of formation' which would render it well-formed but false. The scientist does not mind whether we say that 'This electron has both a definite position and a definite momentum' (S) is meaningless or false; he just wants us not to use this sentence. A satisfactory formalisation could treat it either as ill-formed or as well-formed but inconsistent with the axioms. What Smart seems to ignore, in this and other examples, is that $\sim S$ as well as S is intended to be prohibited. There is surely some point in distinguishing between 'Virtue is not square' and 'Oranges are not square' since, even if we accept the first as true, we can substitute any shape-predicate in the first, but not in the second, *salva veritate.* It is not obvious that Smart's recipe would ever leave us without apparent sentences which we would want to call 'meaningless' rather than 'false'. We *could* adopt different conventions but it is not clear what bearing this has on the question whether two sorts of nonsense can be distinguished within our present conventions.

The symposium 'Causing Voluntary Actions' is full of stimulating suggestions. Joel Feinberg opens with criticisms of Hart and Honoré; he argues that there is no inconsistency in saying that a voluntary human action is caused. He produces examples to show that there are many more ways of causing someone to do something than by compelling, persuading or inducing him to do it and he attempts to remove some of the confusions by distinguishing between *productive* and *explanatory* causal citations. Keith Donellan goes some way with this but urges that, nevertheless, there may be important differences between the causes of voluntary actions and the causes of other events. Keith Lehrer attacks Feinberg's interpretation of Hart and Honoré but argues that, even if we accept Feinberg's thesis, there is reason to doubt that determinism is consistent with our commonsense beliefs about voluntary actions. Feinberg does not wish to deny the first objection and does not claim to have dealt with the second.

Nicholas Rescher, in a symposium on 'Evaluative Metaphysics', puts a case for the existence of distinctively metaphysical values; it is possible to say that certain states of affairs have intrinsic merit which is not ethical or aesthetic or practical but which forms a basis for such values. Lewis White Beck and Thomas Patton both argue, with some justification, that Rescher has not established that metaphysical value is distinct from ethical value. Patton is also inclined to regard Rescher as playing an idle game depending upon an insufficient analysis of the supporting examples; further analysis would reveal that the valuations in question are not independent of the 'realm of human purposes or interests'. Rescher optimistically concedes that this thesis needs more detailed consideration.

The final symposium, 'Things and Qualities', is extremely complex and defies adequate summarising here. Herbert Hochberg considers various ontological analyses of things and facts centring round the problems of universals and individuation. He defends a 'combination of qualities' analysis, chiefly against a 'bare particular' analysis but also against a 'combination of instances of qualities' analysis, leaning towards Bradley and away from Russell, Moore and Gustav Bergmann. Richard Severens points to difficulties in the idea of predication arising on all three views and J. M. Shorter argues that they are either unintelligible or compatible with one another. Hochberg replies that the difficulties raised by Severens are, on the whole, irrelevant and concludes with a spirited defence of metaphysics against an underlying antimetaphysical strain he sees in Shorter's approach.

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Analogy in Quantum Theory: From Insight to Nonsense*

by MARIO BUNGE

Analogy, like the Indian monkeys, is found in every household and everyone admires its fertility but nobody examines it carefully and no one trusts it. The ubiquity and fruitfulness of analogy in hypothesis formation are so obvious that they hardly need be extolled.[1] We all remember that in many respects the molecule behaves like a hard sphere, that the atom used to be pictured as a mini-solar system, that the 'magic' atomic nucleus may be likened to a closed atomic shell system, and that the photon can put on a particle-like face. But we also know that, on closer inspection, these and most other analogies in physics proved to be just that—analogies not literal descriptions. We have learned to tolerate and even encourage daring analogising in the search for new ideas—which is a way of getting extra mileage with the old ones. But we have also learned to mistrust analogy both as a constituent of theories and as an index for their evaluation: we want to go beyond resemblances and we like to think that we do not accept a theory just because it can be understood in terms of familiar ideas.

This ambivalent attitude towards analogy—that prolific but superficial matron—seems prevalent in all scientific fields. Except in quantum theory, where the analogies drawn half a century ago have not yet been forced to retire although they have long ceased to procreate anything but confusion. In particular the analogies between quantum systems and classical particles and waves have become a stumbling block preventing a consistent interpretation of the theory. An aim of the present paper is to vindicate this claim. The upshot will be threefold: (1) an emphasis on the originality of the quantum theory and an invitation to understand it in its own terms; (2) an elimination of the metaphysical doctrines of dualism and complementarity, and (3) the discarding of the thesis that scientific interpretation and explanation are essentially analogical. But before going into business

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* Received 1.vi.67

[1] For a wealth of examples see H. Metzger, *Les concepts scientifiques*, Paris, 1926, M. Dorolle, *Le raisonnement par analogie*, Paris, 1949, G. Polya, *Mathematics and Plausible Reasoning*, 2 vols., Princeton, 1954, and Society for Experimental Biology, *Models and Analogues in Biology*, Cambridge, 1960.

we must cast a glance at the concepts designated by the ambiguous term 'analogy', the more so since they are often handled in analogical ways.

I. ANALOGY: SUBSTANTIAL AND FORMAL

When faced with novelty we spontaneously start to oscillate between two poles: we try to find out what the new thing looks like and what it is unlike. Taking cognisance of resemblances and differences is the beginning of knowledge. Now two objects *A* and *B* are alike to some extent either if they share several properties, as in the case of any two perceptible things, or if the constituents of *A* and their mutual relations match some of those of its analogue *B*, as in the case of a body and its conceptual representation as a point set. While in both cases a partial identity of sorts is involved, in the first case the resemblance, similarity or analogy may be called *substantial* whereas in the second case it may be said to be formal.[1] Water waves and sound waves are substantially analogous to each other (both are pressure waves) and they are formally analogous to light waves (they satisfy similar laws). On the other hand the flows of fluids, electricity and heat are formally but not substantially analogous to one another. Clearly, if two objects are substantially analogous then they are formally analogous as well but not conversely: not even complete formal analogy, i.e. isomorphism, implies substantial similarity.

Whether substantial or formal, analogy can be weak or strong, shallow or deep. The substantial analogy between the individuals of a species is strong, and so is the formal analogy between the optical and the electron microscope. On the other hand the substantial analogy between members of different phyla is weak, and so is the formal analogy between light and electrons. If two things share all their properties then they are identical: substantial identity is then a particular case of substantial analogy. Likewise if the formal analogy between two objects is complete, then they are formally or structurally identical no matter how much they may differ in nature. A comparative concept of substantial analogy can be introduced in this way: *A* resembles *B* more than it does *C* if the set of properties shared by *A* and *B* is more numerous than the set of properties shared by *A* and *C*. And several numerical concepts of substantial analogy have been introduced in numerical taxonomy.[2] As to formal analogy, it can be elucidated as homomorphism: *A* and *B* may be said to be formally analogous if there is a homomorphism either from *A* to *B* or conversely. (A homomorphism of *A* into *B* is a relation-and-operation preserving mapping from all the components of A into some of the elements of B.) If this correspondence holds both ways it is called an isomorphism. A more precise characterisation requires specifying the nature of the objects involved.

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[1] The substantial-formal distinction is found in Metzger, *Les concepts scientifiques* and in M. Bunge, *Scientific Research*, 2 vols., New York, 1967.

[2] P. H. A. Sneath and R. R. Sokal, 'Numerical Taxonomy', *Nature*, **193** (1962), 855.

Whether substantial or formal, analogy is a similarity relation, for it is reflexive and symmetric. But, since it weakens with 'distance', analogy is not transitive except when complete. Therefore, save in the limit case of (substantial or formal) identity, analogy is not an equivalence relation. As a consequence, partial analogy does not lead to the grouping of objects into clear-cut and disjoint equivalence classes: it is incurably hazy. Whence its heuristic power and the decline of its power with the maturation of science. Be that as it may, if we want to attain some clarity about this somewhat unclear yet important concept, we must handle it in a non-metaphorical way, particularly in view that most of the literature on the subject[1] takes the analogy-proportion metaphor for an analysis of analogy. (The Greek ἀναλογία meant proportion or equality of ratios, i.e. $A : B = C : D$.)

Any assertion to the effect that the newborn B is similar to the old hand A, either substantially or structurally, is of course a *statement* of analogy. Analogy statements may be true or false to some extent and by themselves they make neither trouble nor peace. On the other hand a jump from the conjunction of 'A and B are analogous' with a statement about A, to a statement concerning B, is an analogical *inference*. (This elementary point had to be made because much of the contemporary literature on analogy neglects it.) And arguments from analogy may be fertile but they are all invalid: their success, if any, does not depend on their form but on the nature of the case—whence there can be no logic of analogy.[2] In any case, if the statement of analogy occurring among the premises of an analogical inference is one of substantive analogy, then the argument concludes to the possession by B of some or even all the properties A is assumed to have. (Notice the analogy with induction, in which the quantification is over individuals not over predicates.) And if a formal analogy occurs among the premises, then the conclusion of an analogical inference will state that the corresponding pattern will be similar or even identical to the original one.

In either case the conclusion of an argument from analogy is inconclusive: it is a conjecture that must be subjected to tests other than those validating the analogy premise. If the hypothesis proves false we shall conclude that the inference was not only invalid but also barren. But if the hypothesis proves (sufficiently) true, we shall rejoice in the fruitfulness of the (invalid) inference and in the depth of the analogy statement at its base. The greatest reward for such a venture will be finding that the similarity was complete: that it was not 'a mere analogy' but an actual identity, or that it was not 'a mere form similarity' but a structural identity or isomorphism.

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[1] This holds in particular for Dorolle, n. 1, p. 265, and M. Hesse, *Models and Analogies in Science*, Notre Dame, Ind., 1966.

[2] See Bunge, *Scientific Research*, vol. ii, ch. 15, sec. 1.

The uses of analogy in science are essentially the following. *Heuristic use*: for classing, generalising, finding new laws, building new theories, and interpreting new formulas. *Computational use*: for solving computation problems by handling analogues (e.g. electrical models of mechanical systems). *Experimental use*: for solving problems of empirical test by handling analogues, particularly replicas and simulates (e.g. experimental stress analysis of steel bodies on translucent plastic replicas). We shall focus here on the first function of analogy, as found in quantum theory. Thus the assertion that the state vector of a microsystem represents a real wave (the original de Broglie-Schrödinger interpretation) was jumped at from the formal analogy between the quantum-mechanical state equation and the classical wave equations: it was an analogical inference based on an analogy of the formal kind. In the early days of the theory this interpretation was taken as literal not metaphorical, i.e. the formal analogy was taken as indicative of a substantial analogy. Shortly thereafter, by proposing his stochastic interpretation, Born tacitly showed that no substantial similarity was to be concluded from the formal analogy: that the state vector does not represent a peculiar substance spread over the whole available space but that it represents the state of the system. Granted, without the analogy wave mechanics might not have been born and the so-called diffraction of matter waves would not have counted as a decisive empirical confirmation of it. But the survival of this interpretation alongside the stochastic interpretation (in turn cast in a corpuscular language) is responsible for many of the conceptual (noncomputational and nonempirical) difficulties of the quantum theory. This will be shown in the sequel.

2. ANALOGIES ARE BOUND TO BREAK DOWN EVEN IF INITIALLY FERTILE

There is no question that analogy can be fruitful in the preliminary exploration of new scientific territory, by suggesting that the new and unknown is, in some respects, like the old and known. If B behaves like A in certain regards, then it is worthwhile to hypothesise that it does so in other respects as well. Whether or not the hypothesis succeeds, we shall have learned something, while nothing will have been learned if no hypothesis at all had been formulated. Should an analogy hypothesis pass the tests, we shall know that A and B are indeed similar either substantially or formally. And if the analogy fails utterly, we shall realise that some radically new ideas are called for because B is in some respect radically different from A. But unless the analogy is extremely specific or detailed, chances are that it will hold to a first approximation for, after all, our conceptual outfit is limited and no two concrete systems are dissimilar in every respect. The question is to decide what to stress at a given stage of research: whether resemblance or difference should be emphasised.

The elementary treatment of the scattering of light by electrons (Compton effect) is a typical example of the initial triumph and final failure of analogy as a method for handling newcomers. By feigning that both the electron and the photon are particles (an assumption absent from the advanced treatment), the problem can be reduced to a two body elastic collision and the formula for the frequency shift of the scattered radiation can be obtained—as long as one is willing to put up with a frequency that is incomprehensible in the context of mechanics. That is, if we restrict our attention to momentum exchanges and neglect all the other questions, then the photon-ball analogy—which, though baffling, is both substantial and formal—proves fertile. But it breaks down as soon as further questions are asked, some of which will receive wrong answers suggested by the analogy while others will receive no answer at all for they are not covered by the analogy. (Thus the analogy will suggest asking what the photon mass is, the wrong answer being that it must be $h\nu/c^2$ since its momentum equals $h\nu/c$ and its velocity is c. Which is nonsense, for there is no relative mass without a rest mass, and no rest mass without a rest frame—not to mention the nonexistence of photon equations of motion. On the other hand the mechanical analogy will not help us compute the scattering cross section, which requires the consideration of some of the electromagnetic and quantum-mechanical properties of the electron and the photon.)

Analogy, then, is double-edged. On the one hand it facilitates research into the unknown by encouraging us to tentatively extend our antecedent knowledge to a new field. On the other hand, if the world is variegated then analogy is bound to exhibit its limitation at some point, for what is radically new is precisely that which cannot be fully accounted for in familar terms. This seems to be the case of the analogies that helped build the quantum theories, particularly the particle and the wave analogies: they seem to have reached their breaking point long ago. No doubt, in the beginning it was just human to seek inspiration in classical physics, making the pretence that electrons and photons behave sometimes as particles and at other times as fields: there was no help other than analogy for assigning wave mechanics and matrix mechanics (and later on quantum electrodynamics

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as well) some physical meaning. But in the meantime we should have learned at least two lessons. The first is, that the particle and the wave analogies are weak and moreover mutually inconsistent. The second, that the entities the quantum theories are about behave in an original way, i.e. according to nonclassical laws—whence they can be neither classical bodies not classical fields. Therefore it is high time we acknowledged that the quantum theories should get rid of those classical analogues and should recognise that they are concerned with *sui generis* things deserving a new generic name—say, *quantons*.[1]

3. THE WAVE-PARTICLE DUALITY IN OPTICS

Several wave theories of light reigned in optics from the time of Young, Fresnel and Cauchy to the birth of the photon hypothesis in 1905. From here on until the creation of quantum electrodynamics in 1927, two mutually inconsistent bodies of ideas were used to account for the facts of optics: Maxwell's field theory, and a set of hypotheses (hardly a hypothetico-deductive system) clustering around the photon hypothesis. The nature of light was thought to be dual and this duality was often regarded as irreducible.

Quantum electrodynamics (henceforth QED) was built with the purpose of incorporating the wave-particle duality into a consistent body of ideas, thus overcoming the manicheism of the previous period. It is still widely believed that QED did succeed in incorporating that duality, mainly because of the field quantisation and because the theory assigns the photon both a linear and an angular momentum (or rather quantal analogues of such). But of course this does not establish that QED regards the photon as a particle. First, because QED denies it the two defining corpuscular properties, namely a sharp localisation—hence a definite path—and a mass. Second because, as a consequence, QED contains no equation of motion proper: all of its basic equations, including the commutation relations, are field equations, none of which entails photon trajectories in ordinary space. (True, any formula for the time rate of change of a dynamical variable is called an equation of motion, but this is just a metaphor, for there need be no motion involved.) Third because the properties QED assigns the radiation field are nonmechanical—e.g. the electric and magnetic components, the phases, and the frame-independence of the field propagation speed.

If anything, QED has remained closer to Maxwell's field theory than to mechanics: after all, it is essentially the quantisation of the former. The

[1] M. Bunge, *Foundations of Physics*, New York, 1967.

quantum properties of the field should not be mistaken for mechanical or corpuscular properties of it. Thus the fact that the photon momentum can be added to the mechanical momentum of a chunk of matter, to yield a conserved quantity, does not prove the mechanical nature of photons any more than it proves the electromagnetic nature of matter; likewise the possibility of adding energies of different kinds does not prove their identity with mechanical work. All this shows is that the momentum-energy four-vector, unlike mass and charge, is a nonspecific property, i.e. one characterising every physical system known heretofore. Likewise the fact that the field energy can be decomposed into the energy of radiation oscillators does not prove that the field is a mechanical system but rather that the hamiltonian formalism is noncommittal and lends itself to mechanical analogies that are sometimes misleading.[1] If we cast different theories into one and the same mathematical framework—say hamiltonian 'dynamics'—then we are bound to produce formal analogies. But then we should not hypostatisise this mathematical similarity by exclaiming: Lo and behold, mother Nature is all mechanical (or all electromagnetic)!

In short, the wave-particle duality that had broken the unity of electromagnetic theory in 1905 stimulated the creation of another theory, QED, that did away with that duality. Even in its most sophisticated versions QED is a field theory containing no hypothesis about the corpuscular nature of photons. The optical duality is then a relic of the 1905-1927 interregnum, a remains serving mainly to mislead students into believing that light is at the same time undulatory and non-ondulatory or, even worse, that it is so devoid of character that it will look what the omnipotent observer chooses to.

4. THE WAVE-PARTICLE DUALITY IN MECHANICS

The wave-particle duality suggested to de Broglie the problem whether matter might not exhibit a similar duality. If the electromagnetic field seemed to have a mechanical side, it was just possible that matter had a field-like aspect as well. 'The idea of such a symmetry was the point of departure of wave mechanics.'[2] That the analogy was extraordinarily fruitful does not prove that it was substantial rather than formal.

It was fortunate that de Broglie and Schrödinger did not seem to realise at the time that the formalisms of Hamilton and Hamilton-Jacobi are generous enough to harbour almost any physical theory, from mechanics to thermodynamics. Had they known this they might not have marvelled at the analogy between Fermat's optical principle and Hamilton's mechanical principle and they might consequently have missed inventing wave mechanics. We would have had to wrestle with the less pictorial matrix mechanics and we would have been spared the mistake of believing that quantum mechanics deals with waves of a certain kind (matter waves).

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[1] M. Bunge, 'Lagrangian Formulation and Mechanical Interpretation', *American Journal of Physics*, **25** (1957), 211, and *Foundations of Physics*, ch. 2, sec. 6.

[2] L. de Broglie, *Matière et lumière*, Paris, 1937, p. 171. See also E. Schrödinger, 'Quantisierung als Eigenwertproblem. II', *Annalen der Physik*, **79** (1926), 489.

We have come to realise the originality of quantum mechanics (henceforth QM) because the theory can be formulated without resorting to the heuristics of de Broglie and Schrödinger. Moreover, although when working in this particular formulation (the Schrödinger 'picture') one still uses expressions like 'wave function', 'wave packet', 'wave length', and 'wave equation', one tends to regard these as one-sided classical analogues. The very phrase 'the de Broglie wave associated to an electron' shows that we no longer believe electrons to *be* waves: we tend instead to think of the state vector as a global and basic property of a physical system rather than as a thing or as a specific property like mass. Nor do we believe that the so-called elementary particles are plain corpuscles. The tendency is to think of them as quaint entities having both corpuscular and undulatory properties that are alternatively emphasised or brought to light on different occasions. We shall criticise the latter belief for being analogical, but before doing so we must recall what its grounds were: physicists can be stubborn but they are seldom capricious.

There are two arguments in favour of the wave-particle duality of matter: one is based on a set of experiments, the other on a set of formulas. The experiments involving specks of matter, and which are invoked in favour of dualism, are in turn of two kinds: those in which corpuscular properties seem to come to the fore (e.g. tracks of protons on a photographic plate) and those in which wave-like properties are prominent (e.g. scattering of electrons by a regular array of atoms). But this hardly proves anything about the nature of the microsystems concerned, for the experimental arrangements are macrophysical and the experimental outcomes are described in classical terms—in terms of particles and waves. All this shows is that we can use classical ideas when it comes to the macrolevel. But this we knew all along. (More on this in Section 6.)

On the other hand the usual or Copenhagen interpretation of QM is right in emphasising that the 'wave aspect' and the 'particle aspect' depend on the experimental arrangement. Thus an electron will not exhibit the same behaviour when fired through a diffraction grating as when being immersed in an X-ray field. In short the *likeness* of a quanton to either a particle (classical entity) or to a field ripple (classical entity) can be controlled experimentally—or, as the Copenhagen devotees say in a misleading way, it depends on the 'observer'. Put in a different way: the quanton, that proteic object, when acted on by a macrosystem (e.g. an experimental arrangement) can be shaped either into a (pseudo) particle or into a (pseudo) wave, depending on its interactions with that macrosystem. (Needless to say, the latter is a physical system not a psychophysical one and moreover it need not be an artifact: there are gratings and other filters in nature.) But, of course, likeness does not prove identity.

The Copenhagen school is also right in holding that the individual quanton has by itself neither a corpuscular nor an undulatory aspect, but that the compound system quanton-macrosystem ('system-observer' in the usual misleading terminology) can acquire either aspect, to any degree, depending on the nature of the macrosystem (on the 'observer's decision', in the school's anthropocentric interpretation). Therefore if one persists in using classical pictures one is bound to end up in dualism and in denying the quanton an autonomous existence. In short, dualism and subjectivism are not only philosophical tenets inherited by the Copenhagen school[1]: they are also consequences of *classicism*—of an analogical way of thinking.

Fortunately there are measurements, such as the spectroscopic ones, that do not concern a quanton overwhelmed by a macrosystem but concern quantons in free space. The results of these measurements confirm QM and QED to a good approximation. Since these theories can be cast without ever using the concepts of particle and wave, save analogically, those empirical results suggest that the 'particle aspect' and the 'wave aspect' are instrument-dependent (not observer-dependent or subjective) and are therefore not indicative of what the free quanton really is. In other words, paradoxical as it may seem, experiment alone is incapable of proving or disproving the dual nature of quantons, particularly if its outcomes are described in a classical language. In order to ascertain whether the quantum theories countenance dualism we must analyse the theories themselves. If the theories are in fact dualistic in addition to being true, then dualism will be upheld—otherwise rejected.

5. DUALISM: INCONSISTENT AND AD HOC

In order to find out whether dualism is built into the quantum theory one must analyse the formulas of QM and QED rather than remarks on them—and not just some conveniently chosen formulas but the basic ones, i.e. the axioms of these theories. Unfortunately this is seldom done: the usual procedure is to pick precisely those formulas that seem to confirm the dualistic tenet, as if this guaranteed the dualistic interpretation of all the remaining formulas of the theory. In any case, the main formulas usually invoked in support of dualism are the de Broglie equality '$p\lambda = h$' and the Heisenberg inequality '$\Delta x \,.\, \Delta p \geqq h/4\pi$'. However, λ hardly qualifies as a wavelength for, as Landé has emphasised, the wavelength is Galilei invariant while the momentum p is not. And the dualistic interpretation of the second formula is inconsistent with Born's stochastic interpretation of the state vector, which entails that 'Δx' (and likewise 'Δp') designates the standard deviation or statistical scatter around the mean value, a stochastic concept that has little if anything to do with wave packet sizes, apertures of slits in diffraction arrangements, and other items of the dualistic interpretation.[1]

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[1] For the philosophical ancestry of QM, see M. Bunge, 'Strife about Complementarity', this *Journal*, **6** (1955), 1, 141 and 'The Turn of the Tide', in M. Bunge, ed., *Quantum Theory and Reality*, New York, 1967, as well as M. Jammer, *The Conceptual Development of Quantum Mechanics*, New York, 1966, ch. 7, sec. 2.

Yet the standard textbook will make no effort to tally the preceding formulas with Born's stochastic interpretation, which they contradict. In particular, it will waver between these mutually incompatible interpretations of 'Δx' and 'Δp': the objective position (momentum) indeterminacy of the *particle*, the spatial (spectral) width of the *wave* packet associated to the *particle*, the size of the disturbance caused by the apparatus on the *particle* position (momentum), and our subjective uncertainty concerning the actual position (momentum) of the *particle* diffracted (*wave*-wise) through a slit. It is seldom realised that this erratic behaviour is inconsistent and therefore unscientific. Nor is it usually pointed out that every one of these interpretations is *ad hoc*, in the sense that it is arbitrarily forced upon the symbols concerned without any warrant for it. Indeed, firstly, the postulates of QM do not state that quantons are particles—nor do they state that they are field chunks. And secondly, no assumptions concerning either an apparatus or an observer are ever made in order to derive the Heisenberg relations from the postulates of QM, whence it is logically impermissible to mention them at all at the level of the general theory: to do it is to incur in a semantical inconsistency.[2]

At other times it is stated that QM consists of two mutually equivalent theories, one of them cast in the particle language, the other in the wave language. This too is a mistake. There are several, not just two, different formulations of QM and most of them are mathematically isomorphous—which does not entail that they are assigned the same meaning. One of these formulations is Schrödinger's 'picture', in terms of a time-dependent state vector; another is Heisenberg's 'picture', in terms of time-dependent dynamical variables. Further formulations are the density matrix and the path integral ones. They are equivalent formulations of one and the same theory and they are not pictures but high-level symbolic constructions. The Schrödinger formulation suggests analogies with classical field theories; the Heisenberg and the Feynman formulations invite analogies with classical particle mechanics; and the density matrix formulation suggests, if at all, analogies with classical statistical mechanics. But none of these interpretations can be carried through consistently: these analogies are formal, they concern form similarities among some, not all, the quantum-mechanical formulas, and certain classical formulas. Moreover, they cannot be carried over to QED. And none of them is dualistic anyhow.

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[1] A. Landé, *New Foundations of Quantum Mechanics*, Cambridge, 1965, and Bunge, *Foundations of Physics*, ch. 5, secs. 4 and 6.

[2] For the concept of semantical consistency see Bunge, *Scientific Research*, vol. i, ch. 7, sec. 7.5, and 'Physics and Reality', *Dialectica*, 19 (1965), 195.

In short the optico-mechanical analogy, which used to be a fertile working hypothesis, should now be abandoned for it has outlived its purpose by becoming a source of confusion.

6. THE RISE AND FALL OF COMPLEMENTARITY

In the mid-1920s the most advanced physicists believed they had to put up with two dualities: the alleged dual nature of the electromagnetic field and the possible duality of matter. From this double duality there was but a short step to the sweeping ontological conjecture that every physical entity has both corpuscular and undulatory aspects. This is the thesis of *general dualism*; it is a metaphysical hypothesis for it concerns the basic nature of every existent. When QM was built, Heisenberg's scatter ('uncertainty') relations were interpreted in the light of dualism: they were made into a precise and striking illustration of dualism. Bohr's *complementarity principle*—which, like Mach's principle and the Monroe doctrine, was never stated unambiguously, let alone understood clearly—was at the same time a specification and a reinterpretation of general dualism. It was a specification or particularisation for, in addition to stating the duality, it asserted that the more one of the two aspects is enhanced the more its complement is blurred: the more *yin* the less *yan* and conversely.

But, unlike the ontological thesis of general dualism, the complementarity principle claimed to concern the subject-object complex rather than autonomously existing microsystems. Indeed, the orthodox formulations of the principle do not assert that the corpuscular and the undulatory traits of a microsystem balance one another. Instead, they state that what can be complementary is either a pair of macroscopic experimental arrangements (the observer included) or a pair of descriptions of the outcomes of operations conducted with the help of such laboratory setups or, finally, a pair of concepts.[1] Complementarity, in short, strengthens dualism by rendering it slightly more precise—though not precise enough—but on the other hand it weakens duality by failing to attribute it to nature: things in themselves, e.g. atoms in free space, would not have a dual nature—moreover they would be just figments of an imagination not disciplined by the subject-centered philosophy of Copenhagen.

Moreover, since the experimental arrangements and their results are supposed to be classically describable, the complementarity principle remains on this side of QM and QED: strictly speaking it is not a quantum-theoretical statement insofar as it does not concern microsystems. If regarded as a principle of either QM or QED, then it comes in contradiction with the assertion that microsystems satisfy non-classical laws and must therefore be described and explained in non-classical terms. But strictly speaking the complementarity principle is not a principle, for it entails nothing. In fact, no theorem follows from it. In order to prove a theorem in quantum theory one processes a bunch of axioms, usually in conjunction with a set of specific hypotheses concerning, say, the number of microsystems and their mutual actions—in short, one adds a definite model to the general assumptions: see section 9—but one does not use the complementarity principle, which is much too sweeping and vague to entail anything. (In particular, this pseudoprinciple finds no application in QED, for the statistical spreads of the components of the electromagnetic field defy a corpuscular interpretation.) The complementarity pseudoprinciple is then neither a principle nor a theorem—nor is it as general as usually claimed, for it does not hold for fields. And in the advanced quantum theory of 'particles' (second quantisation) the field is treated as the primordial thing. Thus in the case of electrons, or of mesons, the matter field is regarded as the primary entity whereas the 'particles', or rather the particle-like entities, are just field quanta, i.e. field chunks. (An eigenvalue of the occupation number operator represents the number of entities in a given state, and these entities—the field quanta—are not classical particles.) In other words, any second-quantised theory is closer to a classical field theory than to classical mechanics even though it can be cast in a hamiltonian or a lagrangian framework. Consequently there is no place for complementarity in the more sophisticated areas of the quantum theory. Nor is there, *a fortiori*, in any of the phenomenological theories—such as the scattering matrix formalism—that eschew a detailed description of the field.

What then, apart from authority, explains the survival of complementarity? The main reason seems to be its great usefulness. Indeed, complementarity explains many difficulties away and it accounts for experiments of two kinds: gedankenexperiments that have never been carried out and real experiments that have never been computed in quantum-theoretical terms. The first point is obvious: once the pseudo-principle is accepted, it can be used to consecrate obscurities and inconsistencies, much as the mystery of the trinity subsumes many minor mysteries. As to the experiments that allegedly illustrate the principle, they are in fact either imaginary or still beyond the scope of the theory. Among the former one finds Heisenberg's gamma ray microscope and Bohr's shutter experiment in his discussion with Einstein. Since they have no confirming power we may leave them aside. Among the experiments of the second kind, the diffraction experiments stand out. Unfortunately the diffraction by a single slit has been calculated only for an infinitely long slit and a monochromatic de Broglie 'wave'. Moreover the available computation is approximate and its result conflicts head-on with the Heisenberg inequalities[1]—which of course cannot be held against the latter. *A fortiori* the much-discussed double slit experiment has never been computed exactly in QM, let alone in QED. (On the other hand, and oddly enough, it can be explained in a purely corpuscular fashion, with the help of Bohr's classical quantum theory, as an effect of the periodicity of the scattering lattice.[2]) True, a few formulas are used in the qualitative discussions of these experiments, but they are taken from the general theory: they do not result from an application of it to those special circumstances. Also, some nice diffraction patterns are shown, but they are either taken from real (but so far uncalculated) experiments or they are borrowed from classical optics. In short, the discussion of these experiments in terms of complementarity is verbal and analogical. Consequently complementarity is not part and parcel of the quantum theory.

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[1] N. Bohr, 'Kausalität und Komplementarität', *Erkenntnis*, **6** (1936), 293, and P. Frank, 'Philosophische Deutungen und Missdeutungen der Quantentheorie', *Erkenntnis*, **6** (1936), 303. For further sources see n. 1, p. 273.

In conclusion complementarity, reasonable though it looked in the early days of the quantum theory, when people thought in terms of classical pictures, has now exhausted whatever potency it may have had: it has become an excuse for obscurity and inconsistency.

7. TOWARDS A LITERAL INTERPRETATION OF THE QUANTUM THEORY

A number of physicists have felt that the concepts of wave and particle do not belong in QM because they are classical metaphors. Thus Schrödinger held at one time that wave and particle are 'images we are forced to keep because we do not know how to get rid of'.[1] The impression that these are just metaphors, images or visual props, is reinforced by the fact that they fail to occur in any careful attempt to formulate QM in an orderly fashion, i.e. axiomatically. Indeed, they occur neither as undefined concepts nor as derived ones[2]; hence they should not be allowed to figure in the theorems either. The main reason those classical analogues still play an important role in foundations discussions of QM and even QED seem to be inertia and partisan spirit.

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[1] G. Beck and H. M. Nussenzveig, 'Uncertainty relation and diffraction by a slit', *Nuovo Cimento*, 9 (1958), 1068.

[2] See Landé, n. 1, p. 274.

Although most of us realise that the quantum theories chart new territory, we persist in trying to understand it in classical terms—much as Cartier called Lachine a certain place in Montréal because he thought he had arrived in China. We find it handy to call ψ a *wave* function and to draw pictures of wave fronts—only to point out that the 'wave' is a complex function and that its main duty is to inform us where the *particle* is likely to be found. We find it intuitive to call the Schrödinger equation a *wave* equation—only to add that it governs the propagation of ψ in a $3N$-dimensional space. We find it convenient to speak of the *diffraction* of *particles* by a crystal (why not of the collision of waves?) and of the phase shift of the *particle's* associated *wave*, produced by an external field, We thus keep piling inconsistencies hoping that the complementarity principle, like confession, will absolve us.

But we can do even better: with hindsight and ingenuity classical physics can be restated in ways reminiscent of the quantum theory. Thus particle mechanics can be formulated, within the Hamilton-Jacobi framework, as a field theory dealing with the propagation of a fictitious wave built with the help of the solution of the equation of motion. If a classical analog of second quantization is required, it can be supplied as well.[3] In short, just as nearly every nonrelativistic quantum-mechanical formula can be misinterpreted in classical terms, so nearly every classical formula can be restated in a (pseudo) quantal fashion. Unfortunately, at this late hour few of these analogies are more than formal games: they seldom yield new insights and they never lead to new specific predictions. Trying to discover the quantum in classical physics is as hopeless as trying to read QM and QED in classical terms.

The evasion from this tangle of inconsistencies, obscurities and metaphors is simple enough, namely to regard microsystems as altogether heathen individuals. Consequently they should be rechristened with heathen names, such as *quanton* (family name) and the genera names *hylon* (from ὕλη, matter) and *pedion* (from πεδίον, field). Even the names of the theories concerned could be profitably changed, for instance into *hylonics* (= quantum mechanics), *pedionics* (= quantum field theory) and *quantics* (the union of the two). After all, the quantum theory is a successful upstart and every successful upstart needs a new name that will not betray its origin.

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[1] E. Schrödinger, *Mémoires sur la mécanique ondulatoire*, Paris, 1933, p. xiv.

[2] See Bunge, *Foundations of Physics*, ch. 5, and 'A Ghost-Free Axiomatization of Quantum Mechanics', in M. Bunge, ed., *Quantum Theory and Reality*, New York, 1967.

[3] R. Schiller, 'Relations of Quantum to Classical Physics', in M. Bunge, ed., *Delaware Seminar in the Foundations of Physics*, New York, 1967.

Of course this is not just a question of names: classical concepts must be either reformed or wiped out from the quantum theory unless they happen to function in it just as they did in classical physics. Thus in the elementary theory the concepts of mass, charge and electromagnetic field are classical. On the other hand what is usually called the operator representative of the particle position is nothing of the sort: 'x' designates just a point in the configuration space and, unless the probability distribution is given, a specific value of x tells us nothing about the hazy location of the quanton. Only the quantum-mechanical average of x, built with the help of that probability density, is the analogue of the classical position co-ordinate, as shown by both the formal structure of the concept and by the formal analogy between the corresponding equations of motion. The foundations of QM and QED can and must be laid down without the help of either classical analogies or of more or less ideal measurements—just as thermodynamics is formulated nowadays without the caloric fiction and without resorting to engine cycles.

It is only when looking for classical or semiclassical correspondence limits, and when applying the general theory to particular cases, that we are entitled to seek again inspiration in classical physics. We shall try to find quantum renditions of classical formulas and, conversely, classical analogues of quantum formulas. (An expression C may be called a *classical analogue* of a quantum-theoretical expression Q if and only if either C and Q play homologous roles in formally analogous formulas, or if C is a correspondence limit of Q.) And we shall also avail ourselves of classical physics when hypothesising quantum hamiltonians or lagrangians. Borrowing them from classical physics and rewriting them in quantum-mechanical terms with the help of heuristic rules is honest practice provided the new hamiltonians are not read in classical terms. (However, stealing does not pay in two important cases: when the classical hamiltonian cannot be symmetrised in an unambiguous way, and when radically new interactions, such as exchange forces, are at stake.) In any case, whether or not a quantum formula has a classical analogue, it should not be interpreted in classical terms but as dictated by the interpretation axioms of the theory.[1]

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[1] For the form-content difference see M. Bunge, 'The Structure and Content of a Physical Theory', in M. Bunge, ed., *Delaware Seminar in the Foundations of Physics*.

And these assumptions (also called 'correspondence rules' and sometimes 'operational definitions') should be literal rather than metaphorical, as well as objective rather than operator-centred.

A physical theory is assigned a *literal and objective interpretation* by assigning every one of its referential primitive symbols a physical object—entity, property, relation, or event—rather than a mental picture or a human operation. Thus the state vector is not properly interpreted as a wave field (in the style of the electromagnetic field) or as an information carrier, but as representing the state of the system concerned—just as in statistical mechanics every state of a N-body system is mapped on to a point in the corresponding $6N$-dimensional phase space. The fact that the evolution of the state of the system is described (in the Schrödinger formulation) by an equation reminding us of a wave equation, does not prove the existence of a substantial analogy, the more so since many alternative metaphorical interpretations are conceivable.[1]

As with foundations so with their applications: analogical thinking, however fertile as a starter, eventually becomes confusing. A case in point is many-body theory with its twenty or so quasiparticles and pseudoparticles. Thus on an analogy with the electromagnetic field it was conjectured that sound waves are quantised, i.e. that the kinetic elastic energy of a body equals an integral number of sound quanta or phonons. This hypothesis was eventually expanded into a full-blown theory which found abundant empirical confirmation. But the analogy, though fruitful, is shallow: while a photon is a chunk of electromagnetic field and may acquire an autonomous existence by freeing itself from its source, a phonon is no such independent thing: it is a property of a complex system: there are no free phonons. Similarly with the other quasiparticles and with the so-called resonances in elementary particle theory: they are states of things rather than independent things. To suggest that they behave *like*, or *as if* they were, particles was insightful, for it started the conceptual machinery. To maintain that they *are* particles is nonsense, for they fail to exhibit the defining characteristics of a particle, namely independent existence, a sharp localisation, and a mass. Yet the current physical literature teems with such analogical nonsense.

8. RIGOROUS INTERPRETATION AND EXPLANATION: LITERAL NOT METAPHORICAL

Poets, theologians and occult scientists resort to metaphors and analogies on the ground that they handle subjects defying direct description or perhaps even rational understanding. Teachers use the same trick for a different reason, namely in order to bridge the chasm between the unknown and the familiar. Who among us has resisted the temptation to picture electrons sometimes as balls and at other times as wave packets? Yet we know that such metaphors and analogies are at best didactic props, at worst didactic traps, always *Ersätze* for the real thing. We therefore try to avoid them in research. We wish science to be concerned with what things are not with what things look like: science is neither poetry nor theodicy nor occult science. While we are willing to let analogy guide our preliminary explorations (notice the metaphor), we feel it is wrong to allow it to play any role in mature theory: we want this to depict the thing itself rather than something that superficially looks like it. In other words, we want *literal interpretations*—even if they are assigned no familiar visualisations—because we want objectivity. It is only in mathematics that we are interested in mirroring one conceptual structure on to another. The conceptual frameworks of factual science are supposed to mirror (symbolically and partially to be sure) real things not further constructs. To stick to analogy in factual science is just beating around the bush: analogical thinking is characteristic of protoscience (e.g. history) and pseudoscience (e.g. psychoanalysis). Grown-up science is literal as much as it is objective. Its epistemology is consequently realist.

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[1] For a number of metaphorical interpretations of QM, see M. Bunge, 'Survey of the Interpretations of Quantum Mechanics', *American Journal of Physics*, 24 (1956), 272.

A *literal and objective interpretation* of a basic (primitive) symbol s occurring in a physical theory T assigns s a physical object p, be it an entity (e.g. an atom), a property thereof (e.g. the atom angular momentum) or a change of it (e.g. a jump in the angular momentum value). In short, $p = Int(s)$. A literal and objective interpretation of a whole physical theory T will accordingly consist in a mapping $Int\colon S \to P$ of the set S of the basic symbols of T into the set P of their physical partners. If P is wholly borrowed from another field, covered by a theory T' (i.e. if $P \subset P'$, where P' is the set of physical objects referred to by T'), then T will be interpreted in *analogy* with T′. In particular, if T is a quantum theory and T' a classical theory and P is borrowed from T′, then the classical interpretation of T will be metaphorical. And if P has a nonempty overlap with a set of psychological objects such as human dispositions and abilities, say observability, uncertainty, and predictability, then T will be a psychophysical rather than a strictly physical theory.

What holds for interpretation holds for explanation: if the formalism of a physical theory is assigned a literal and objective interpretation, then any explanation contrived with the help of that theory will be literal as well. We will not reject metaphorical explanation altogether: it should be tolerated, *faute de mieux*, in the period of theory construction. Thus it would have been foolish to reject the hydrodynamical analogies of heat and electricity 'flow' in the beginning—as foolish as regarding them as substantial rather than formal. Likewise information theorists did well to profit by the formal similarity between information and negentropy. But it is abusive to reverse the move and reduce entropy increase to loss of human information about the system, for this deprives statistical mechanics and thermodynamics of objectivity. In other words, an interpretation of 'S' in statistical mechanics must assign it an objective physical property not the state of human knowledge. Likewise, if QM and QED are to count as physical theories, then ψ must be assigned a physical meaning in an objective and literal fashion.[1]

The situation, then, is this. If we want to build or learn new theories then we are likely to use analogy as a bridge between the known and the unknown. But as soon as the new theory is on hand it should be subjected to a critical examination with a view to dismounting its heuristic scaffolding and reconstructing the system in a literal way—this being one of the uses of axiomatisation.[2] Once this reconstruction has been achieved, i.e. once we have found out not what the theory looks like but what it is, and not what the referents of theory mimick but what they are, we must refuse to acknowledge any metaphorical explanation in the field covered by the theory, for it will be a pseudoexplanation. To suggest that scientific explanation is metaphorical is to mistake scientific theories for biblical parables or to subscribe to instrumentalism.[3]

9. MODELS

How about models: should they too be regarded as mere heuristic auxiliaries to be discarded after the theory has been built? The answer depends on the sense of the polymorphous word 'model', a term which is as widely used as little analysed in the recent philosophy of physics. There are two senses in which models are indeed an ingredient of physical theories and two others in which they need not be so.

If 'model' is taken to mean *visual representation* or analogy with familiar experience,[4] then clearly not every theory involves a model: thus field theories, whether classical or quantal, are hardly visualisable. And if 'model' is taken to mean *mechanism*—either in a narrow mechanical sense or in a wide sense including non-mechanical mechanisms such as the meson field mechanism of nuclear forces—then some theories do contain models of this kind while other do not. (The former may be called mechanism or representational theories while the latter may be called phenomenological or black box theories.[1]) Consequently the neo-Kelvinist view that every scientific theory contains or presupposes a model consisting in a pictorial representation or analogy, is not adequate. Therefore the associated view, that both scientific interpretation and scientific explanation require visual representations, is false as well.

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[1] See n. 2, p. 278 and K. R. Popper, 'Quantum Mechanics Without "The Observer" ', in M. Bunge, ed., *Quantum Theory and Reality*, New York, 1967.

[2] For the characteristics and uses of physical axiomatics, see Bunge, *Foundations of Physics*, ch. 4, sec. 4.2, and 'Physical Axiomatics', *Reviews of Modern Physics*, **39** (1967), 463.

[3] H. Vaihinger, *Die Philosophie des Als Ob*, 4th edn., Leipzig, 1920, p. 42: 'All knowledge, unless it establishes just actual succession and coexistence, can only be analogical.'

[4] E. Hutten, *The Language of Modern Physics*, London, 1956, pp. 81-82. For pictorial representations see M. Bunge, *Intuition and Science*, Englewood Cliffs, 1962, ch. 3.

On the other hand in a third sense every physical theory *is* a model (of the underlying mathematical formalism) and in a fourth sense every *specific* physical theory (but not every theory) *contains* a model or sketch of its particular referent. (Unfortunately the recent revindications of the role of models in science do not concern the third or model-theoretic sense of 'model', or they confuse it with the fourth sense.) The first claim is obvious in view of the characterisation of physical interpretation given in Sections 7 and 8. Moreover a physical theory is twice a model in the model-theoretic sense: once because every one of its basic signs has a particular interpretation within mathematics, another time because the same sign may have a physical interpretation as well—as is the case of all the referential primitives. Thus in mechanics '*m*' may first be interpreted as a number, then as the inertia of the body it is assigned to. The final interpretation of '*m*' is then the composition of a mathematical and a physical interpretation function: the sign is assigned a number which is in turn assigned a quantity of inertia. *Mutatis mutandis* for the whole symbolism of the theory. Caution: this, the model-theoretic sense of 'model', can be used only in connection with axiomatised theories, for a model of an otherwise uninterpreted factual theory is built by assigning every one of the primitives of the theory a factual interpretation—and one does not know which are the primitives before axiomatising the theory.

As to our second claim, involving a fourth sense of 'model', namely as designating an idealised representation of the physical system, it boils down to this. The general formulas of a theory are nonspecific, so that they do not suffice to solve particular problems, such as finding the trajectory of a missile or the modes of propagation of a wave in a waveguide or the energy levels of an atom. In order to solve any such specific problem a number of specific assumptions and data concerning the particular physical system referred to must be supplied: the number and nature of the parts and their assumed interactions, the constraints and the constitutive equations, the initial and boundary conditions, and what have you. And these subsidiary hypotheses and data, which are adjoined to the generic axioms of the theory to cure their indeterminateness, jointly constitute a *theoretical model* of the concrete system. A model in this fourth sense is then a set of *statements* specifying (roughly) the nature of the referent of the theory in a more precise way than the general (and therefore highly indeterminate) assumptions do. Consequently, notwithstanding a widespread opinion,[1] a theoretical model cannot be *replaced* by a 'formal theory' (meaning a full-fledged factual theory).

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[1] See A. d'Abro, *The Decline of Mechanism*, New York, 1939, ch. xi, and Bunge, *Scientific Research*, vol. i, ch. 8, sec. 5.

Here go some examples of theoretical models in physics: (1) the mass-point and the hard-sphere models of a gas; (2) the Ising model of a body in a condensed phase: an array of atoms or molecules regularly spaced and such that every one of them interacts only with its nearest neighbours; (3) the classical model of a fluid, or even the whole universe, as a continuous medium with given density and stress distributions; (4) the elementary model of an electric current as a one-dimensional current of infinite density; (5) the potential barrier as a representative of an external force and the potential well as a schematisation of an attractive inner force in quantum mechanics. Notice, first, that every such model involves some of the concepts of a given theory, for otherwise it could not be adjoined to it. Second, none of these models is committed to a specific set of law statements: they can occur in widely different and even mutually incompatible theories of a given class (classical and quantal, nonrelativistic and relativistic, etc.). In short, the theoretical model is not part and parcel of the foundations of a general theory: this theory must be on hand, as fully interpreted as possible, if it is going to be applied to a model thereby becoming a specific theory—e.g. a nonrelativistic quantum theory of the helium atom. That is, the model does not contribute, except heuristically, to endowing the general theory with a factual (e.g. physical) meaning. Third, a theoretical model is neither idle nor faithful: it is, or rather it is supposed to be and so taken until further notice, an approximate representation of a real thing.

Whether or not a given theoretical model or representation of a physical system happens to be picturable, is irrelevant to the semantics of the theory to which it eventually becomes attached. Picturability is a fortunate psychological occurrence not a scientific necessity. Few of the models that pass for visual representations are picturable anyhow. For one thing, the model may be and usually is constituted by imperceptible items such as unextended particles and invisible fields. True, a model can be given a graphic representation—but so can any idea as long as symbolic or conventional diagrams are allowed. Diagrams, whether representational or symbolic, are meaningless unless attached to some body of theory. On the other hand theories are in no need of diagrams save for didactic purposes. Let us then keep theoretical models apart from visual analogues.

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[1] M. Black, *Models and Metaphors*, Ithaca, N.Y., 1962, ch. xiii.

10. UPSHOT

In factual science analogy and analogical inference are welcome as theory construction tools. By the same token they are signs of growth, symptoms that the theory is still in the making rather than mature. A mature classical electrodynamics has no need for elastic tubes of force: the field—a non-mechanical substance—suffices for all purposes, the mechanical analogues being regarded as removable appendages. Likewise a mature quantum electrodynamics will need no virtual photons jumping out of electrons and back: it will take the Feynman diagrams as mnemonic devices attached to a computation method rather than as literal descriptions.[1]

When a factual theory attains maturity it involves only literal interpretations and it yields only literal explanations: it does not involve *as ifs*. Surely scientific explanation, if deep, is more than just deduction from laws and data: it will be a subsumption under generalities all right but, among these, some mechanism hypotheses will figure—that is, some assumptions will go beyond external or input-output relations. Yet such explanations in depth, or interpretive explanations,[2] are alien to metaphorical explanations, which are superficial for being limited to similarities and for failing to concern the real thing. Correspondingly the metaphor view of scientific explanation,[3] which has recently been commended in place of the deductive account, is totally inadequate.

It behoves the foundational worker and the philosopher of science (1) to recognize the splendid gifts analogy statements and analogical inferences can make to theory construction, (2) to analyse them rather than describing them in metaphorical terms, (3) to expose the uncritical use of analogies and arguments from analogy in science, (4) to distinguish the constitutive

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[1] M. Bunge, 'The Philosophy of the Space-Time Approach to the Quantum Theory', *Methodos*, 7 (1955), 295.

[2] M. Bunge, *Scientific Research*, vol. ii, ch. 9, sec. 9.4, and 'The Maturation of Science', in I. Lakatos and A. Musgrave, eds, *Problems in the Philosophy of Science*, Amsterdam, 1967.

[3] Hesse, *Models and Analogies in Science*, p. 157: 'the deductive model of scientific explanation should be modified and supplemented by a view of theoretical explanation as metaphoric redescription of the domain of the explanandum.'

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Definition and Discovery(I)*

by FRED WILSON

INTRODUCTION

The end for which the scientist strives is that of ***stating laws.*** The description and explanation of the activities which contribute to this end belong to the context of discovery. Among the important activities of this process is that of ***introducing concepts*** into the vocabulary of the scientist. As the ultimate aim is that of stating laws, the only concepts worthy of introduction are those which serve as means to this end. Or, in other words, among the products of the scientist's activities will be ***significant concepts,*** concepts which (the scientist believes) enter into statements of law. Furthermore, these ***products,*** significant concepts, are ***explicitly defined.*** Or, at least, so the ***definitionalist*** maintains.

I do not propose to defend the definitionalist's thesis in this paper.[1] I shall assume that the language of the scientist contains only (1) undefined and directly interpreted predicates, and (2) predicates explicitly defined on the basis of these.[2] In particular, I shall assume that all those terms which are, as one says, 'operationally defined' are predicates which are explicitly defined. This means that such concepts as 'length', 'weight', 'soluble', 'kinetic energy', 'heat', etc., all gain their meaning by virtue of chains of definitions which 'hook' these concepts to the observational base, to the undefined and directly interpreted terms. In addition it means that all such concepts are, in principle, eliminable. For, the only condition a

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* Received 14.vii.67

[1] For such a defence, see G. Bergmann, 'Comments on Professor Hempel's "The Concept of Cognitive Significance" ', *Proceedings of the American Academy of Arts and Sciences,* **80** (1951), 78-86; reprinted in G. Bergmann, *The Metaphysics of Logical Positivism,* (New York, 1954).

[2] Exceptions (which are not relevant for the present paper) occur in the case of concepts involving real numbers, and in the cases of concepts which refer to the entities of the model of the theory of the structure of matter. See G. Bergmann, 'Sense and Nonsense in Operationism', *Scientific Monthly,* **79** (1954), 210-15; reprinted in P. Frank (ed.), *The Validation of Scientific Theories* (Boston, 1957). Besides these exceptions, there remains disagreement with respect to the two points (1) and (2). The present paper is in part an attempt to remove this disagreement, specifically in favour of (1) and (2). For some other aspects of the defence, see M. Brodbeck, 'Explanation, Prediction, and "Imperfect" Knowledge', in H. Feigl and G. Maxwell (eds), *Minnesota Studies in the Philosophy of Science,* vol. iii (Minneapolis, 1962); and 'Meaning and Action', *Philosophy of Science,* **30** (1963), 309-24; also H. Hochberg, 'Intervening Variables, Hypothetical Constructs, and Metaphysics', in H. Feigl and G. Maxwell (eds), *Current Issues in the Philosophy of Science* (New York, 1961).

definition must fulfil is that the term it defines be in all contexts eliminable in favour of its definiens.

Since these non-primitive predicates are all based on stipulations, they are, to that extent, arbitrary. The *process* of introducing them is not arbitrary, however. For, though the scientist can define words as he pleases, since he is a scientist and therefore (*qua* scientist) only interested in stating laws, he is pleased with only such defined terms as are significant, i.e. as occur in statements of law.

Another assumption which I shall make in this paper is that the basic sense of 'if-then' in terms of which these 'operationally defined' concepts are introduced is that of the material conditional. In other words, I shall assume that, for example, (D) 'x is put in water $\supset$ x dissolves' suffices for analysing 'x is soluble'. To be sure, this has been challenged. But the challenge can be met, I believe, by pointing to the laws which render (D) significant. For example, there is the law that sugar is soluble, i.e. the law that

$$(x)[x \text{ is sugar} \supset (x \text{ is put in water} \supset x \text{ dissolves})]$$

I shall assume that this belief is justified, that the fact that (D) is significant enables one to meet the criticism levelled against it as an analysis of 'soluble'.[1]

The definitionalist's thesis has been challenged by the defenders of 'reduction sentences' so-called. Instead of 'x is soluble' being short for (D), it is, we are told, 'introduced' by the reduction sentence

$$\text{(R)} \qquad (x)[x \text{ is put in water} \supset (x \text{ is soluble} \equiv x \text{ dissolves})]$$

In (R), both 'x is put in water' and 'x dissolves' are already assigned meanings. The predicate 'x is soluble' 'introduced' by (R) is undefined and not assigned a rule which interprets it directly; rather, its 'meaning' is given to it by virtue of its occurring in (R).[2] Thus, the concepts which the definitionalist maintains are assigned meaning by definition chains, the defenders of (R) claim are assigned meaning by chains of reduction sentences.

The scientist engages in activities of introducing concepts. The products of these activities are the concepts of his language. To speak of these *products* is to restrict oneself to *the context of justification.* The definitionalist's is, of course, a thesis about these products; it is a thesis about the *logic* of the concepts introduced. In contrast, the thesis that scientists are interested in only such concepts as are significant, is a thesis about the *psychology* of scientists; it is a thesis of *the context of discovery*.[1]

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[1] Cf. J. Weinberg, 'Contrary-To-Fact Conditionals', *The Journal of Philosophy*, 48 (1951), 17-22; H. Hochberg, 'Dispositional Properties', *Philosophy of Science*, 34 (1967), 10-17.

[2] Cf. R. Carnap, 'Testability and Meaning', *Philosophy of Science*, 3 (1936), 417-71, and 4 (1937), 1-40; reprinted in part in H. Feigl and M. Brodbeck (eds), *Readings in the Philosophy of Science* (New York, 1953), 47-92; and also C. Hempel, *Fundamentals of Concept Formation in Empirical Science* (Chicago, 1952).

If the defender of reduction sentences is offering a thesis opposed to that of the definitionalist, then his thesis must be one which falls in the context of justification. Taking it to be such, the idea of reduction sentences is, I believe, mistaken. At least, so I have argued in detail elsewhere.[2] Indeed, I have argued that the plausibility of reduction sentences arises from a confusion on the part of their defenders; that it arises from a confused desire to have the context of justification (i.e. logic) reflect the context of discovery (i.e. psychology).[3]

I shall assume that these arguments and diagnosis are correct, and shall but merely summarise them in section I of the present paper.

To speak of the *logic* of concepts is to speak of the context of justification. The process which has these concepts as its products is quite another thing. Even though every term in the product is explicitly defined, according to the definitionalist, it does not follow that the definitionalist must say that there is nothing in the process leading to these products which looks like reduction sentences. This paper proposes to show that, even though reduction sentences occur nowhere in the analysis of the products of the scientist's activities, there remain features of the context of discovery, of the process leading to the explicitly defined terms, which bear significant similarities to what have been called reduction sentences.

If I succeed in showing this, then the definitionalist's case will thereby be much strengthened. It will be strengthened in two ways. (1) Assume the appeal to reduction sentences is in fact motivated by a certain desire, namely, the desire to have the context of justification reflect that of discovery. If it can be shown that in some obvious sense reduction sentences (or something like them) have a place in the latter, the definitionalist will have located another reason why the motivating desire should lead one to suppose reduction sentences would occur in the context of justification. (2) By analysing the processes of the context of discovery within the framework of the definitionalist's thesis, certain standard supposed counter-examples to that thesis will have been shown not to constitute counter-examples after all.

Thus, the present paper is both diagnostic and expository: it diagnoses some of the source of the reduction sentence idea; and it indicates how various phenomena of the socio-psychological process of discovery are handled by the definitionalist.

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[1] To be sure, it is a trival thesis; but trivial or not, it is still a *psychological* thesis.

[2] See 'The Notion of Logical Necessity in the Later Philosophy of Rudolph Carnap', chapter 4, in A. Hausman and F. Wilson, *Carnap and Goodman: Two Formalists* (The Hague, forthcoming).

[3] Cf. G. Bergmann, 'Physics and Ontology', *Philosophy of Science*, 28 (1961), 1-14; reprinted in G. Bergmann, *Logic and Reality* (Madison, Wisc., 1964); and N. Goodmann, *Fact, Fiction and Forecast* (Second Edition, Indianapolis, 1965), esp. pp. 46-49.

I

The reduction sentence analysis is, briefly, as follows. Suppose 'P_1' and 'P_2' are predicates which have already been assigned meaning. Suppose the undefined predicate 'Q' is to be introduced. This is done by the reduction sentence

(1.1) $$(x)[P_1x \supset (Qx \equiv P_2x)]$$

Suppose, for some individual *a* that '$\sim P_1a$' is true. Then 'Qa' can neither be affirmed nor be denied. There is thus an area of 'indeterminacy' in the meaning which (1.1) assigns to 'Q'; 'Q' remains meaningless, i.e. undetermined in meaning, for that portion of the universe of discourse for which the antecedent test conditions are not fulfilled.

If one now wished to reduce this area of 'indeterminacy of meaning', one could add a further reduction sentence for 'Q':

(1.2) $$(x)[P_3x \supset (Qx \equiv P_4x)]$$

However, (1.2) cannot be taken as a mere additional stipulation. For (1.1) and (1.2) together entail a synthetic generality (statement of law) which does not contain 'Q', namely,

(1.3) $$(x)[(P_1x \,.\, P_3x) \supset (P_4x \equiv P_2x)]$$

This means that before (1.2) can be added to reduce the number of meaningless sentences involving 'Q', that is, before it can be added to reduce the area of 'indeterminacy of meaning' of 'Q', one must first establish that (1.3) is true.

As our knowledge increases further reduction sentences will be added until there is no 'indeterminacy of meaning' remaining. Every individual in the universe will satisfy the antecedent test conditions of one or more of the reduction sentences for the concept (though, of course, not necessarily the condition mentioned in the consequents). At this point, no indeterminacy would remain, and one could replace the reduced concept by an explicit definition. This is easily seen. Suppose that (1.1) and (1.2) succeeded in completely removing the 'indeterminacy of meaning' of the term 'Q' which they serve to 'introduce'. In that case every individual in the universe will satisfy one or the other or both of the antecedent test conditions; i.e.

(1.4) $$(x)[P_1x \vee P_3x]$$

will be true. But (1.1), (1.2) and (1.4) jointly entail

(1.5) $$(x)[Qx \equiv ((P_1x \supset P_2x) \,.\, (P_3x \supset P_4x))]$$

In the light of (1.5) one can suggest that (1.1) and (1.2) be replaced by an explicit definition for 'Q', namely,

(1.6) 'Qx' is short for '$(P_1x \supset P_2x) \, . \, (Px_3 \supset P_4x)$'

Then (1.5) would be true by definition, and would contain no uninterpreted term. This would serve to make explicit the purely conventional parts of (1.1), (1.2) and (1.4), and to separate these from the factual content embodied in (1.3) and (1.4).[1]

Suppose the original reduced predicate 'Q' were part of further reduction sentences: for example, it might occur in the reduction sentence

(1.7) $(x)[Qx \supset (Rx \equiv Sx)]$

for 'R'. Then (1.5) would enable one to replace the reduced 'Q' by the predicate explicitly defined by (1.5). And so on, for all other contexts containing the original reduced 'Q'.[2]

And this, it is suggested,[3] reflects the actual growth of science. Acceptable scientific concepts have an 'openness of meaning' which enable them to serve as 'crystallisation points' for the growth of scientific knowledge.[4] Reduction sentences show how this is possible (so it is claimed) because their *logic* provides an 'openness of meaning' such that, as new laws are discovered (as, e.g. (1.3)), the scientist can add further criteria (indicators) for applying the concept, these further criteria being laid down in additional reduction sentences (such as (1.2)). Furthermore, their logic shows that, with the growth of knowledge, and the addition of new indicators, the 'meaning' of a concept gradually increases until such a time as it is completely determined. At that time the scientist will give us an explicit definition of it. But (it is suggested) it is unreasonable to ask for an explicit definition (as the definitionalists do) until knowledge of laws has increased to the point where this is possible.

There are three decisive objections to the very idea of a reduction sentence (in the context of justification). The first is the most crucial; the other two depend upon it.

One. The predicates 'introduced' by reduction sentences are literally meaningless, since they are undefined predicates yet are not assigned a rule of interpretation.[5]

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[1] One wants to suggest that a concept is completely defined in case that all individuals are test*able*, rather than test*ed*. That is, it would seem that the truth of (1.4) is not a necessary condition for a term having its meaning completely determined. This difficulty can be by-passed if the defender of reduction sentences changes the 'if-then' which occurs in them from that of the material conditional to a stronger one which supports the subjunctive conditional that 'test*able*' requires.

[2] Cf. R. Carnap, 'Testability and Meaning', in Feigl and Brodbeck, *Readings in the Philosophy of Science*, p. 60.

[3] Cf. C. G. Hempel, *Fundamentals of Concept Formation in Empirical Science*, pp. 28-29.

[4] The two phrases are Hempel's op. cit.

[5] Cf. N. Goodman, *Fact, Fiction, and Forecast*, p. 47, n. 11.

Two. It follows that the syntax-semantics distinction is blurred. Unlike explicit definitions, which are purely syntactical, placing no limitations on how rules of interpretation are assigned to the undefined primitives, reduction sentences are supposed to be stipulations and yet still place restrictions upon how one may interpret the undefined predicates. For, (1.1) requires that *no* rule of interpretation be assigned to the predicate '*Q*' which it introduces.

Three. Consider an individual *a* for which the meaning of '*Q*' is not yet 'determined' by reduction sentences. Then '*Qa*' is syntactically a sentence but is neither true nor false. This means that the customary 'truth-table' techniques cannot be used to explicate the analytic-synthetic distinction, since their use depends upon having the semantics of the language such that every string which is syntactically a sentence is either true or false and not both. As these techniques provide the only known basis for explicating the distinction, it follows that a language in which terms are 'introduced' by reduction sentences cannot explicate the dichotomy between matters of fact and matters of logic.[1]

It is claimed that 'introducing' terms by reduction rather than by explicit definition has the following advantage. In science, acceptable terms are those which serve as 'crystallisation points' for scientific discovery, for the growth of scientific knowledge. Explicit definition cannot, while reduction can, account for this. For, the meaning of explicitly defined terms is settled once for all, while the meaning of reduced terms can be increased (by adding more reduction sentences) as scientific knowledge increases (through the discovery of laws such as (1.3)). Thus, it is said, reduction does, while explicit definition does not, reflect accurately what goes on in the practice of science.

That terms 'introduced' by reduction sentences have an 'openness of meaning' in the relevant sense cannot be denied. (Indeed, that is precisely why they are to be rejected; this 'openness of meaning' requires that the predicate introduced be meaningless (undefined and uninterpreted).) However, the above argument does not establish that this is an advantage over explicit definition. For the argument rests upon a confusion: it supposes that logic must reflect the order of discovery. Our logical analyses of the concepts the scientist introduces belong to the context of justification. There is no reason whatsoever why these analyses should reflect the order in which the scientist discovered the laws which make them significant, and therefore acceptable to the scientist. For logic is one thing, discovery another.[2]

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[1] Carnap claims otherwise in his 'Testability and Meaning', but I have shown this claim to be false in my study, 'The Notion of Logical Necessity in the Later Philosophy of Rudolph Carnap'; see ch. 4, sec. (4.1).

[2] Cf. N. Goodman, *Fact, Fiction, and Forecast*, p. 47.

Furthermore, given the distinction between logic and discovery, it is clear that the definitionalist can account for the fact that concepts acceptable to the scientist have an 'openness of meaning' in the sense that they serve as 'crystallisation points' for the development of scientific knowledge (knowledge of laws). Terms which have an 'openness of meaning' in this sense are simply those which are discovered to be significant. And the scientist finds acceptable only such concepts as are significant. Which gives the definitionalist all he needs against the cited argument in favour of reduction sentences.

The confusion of logic and sociology-psychology on which rests the argument defending reduction sentences, is plausible only if one confuses two kinds of justification. *First*, there is the justification which the scientist gives when he shows a certain activity (introducing concepts) has as its product the end toward which the activity was directed (a significant concept). *Second*, there is the justification which shows that the product (a defined concept) satisfies the canons of logic (it must be eliminable in all contexts) and is as the first justification requires it to be (the laws which make the product, the defined concept, significant, are reasonably confirmed). Justification of the first sort is a matter of the psychology of scientists; justification of the second sort is a matter of logic. And these are two things and not one.

II

Suppose we have the definition

(2.1) 'Gx' is short for '$F_1x \,.\, F_2x$'

The two sentences

(2.2) $(x)[Gx \equiv F_1x]$

and

(2.3) $(x)[Gx \equiv F_2x]$

are, of course, both synthetic, though

(2.4) $(x)[Gx \equiv (F_1x \,.\, F_2x)]$

may, by virtue of (2.1), be treated as if it were analytic.[1] Assume that we already know that (2.2) and (2.3) are true. That means we also know that

(2.5) $(x)[F_1x \equiv F_2x]$

is true. Suppose, now, that we discover the generalisation

(2.6) $(x)[F_2x \equiv F_3x]$

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[1] It is not *literally* analytic, for it is not true by virtue of its (syntactical) form alone. But it may be treated *as if it were* analytic, since the elimination of defined terms produces a sentence which *is* true by virtue of its form alone. Cf. F. Wilson, 'Implicit Definition Once Again', *The Journal of Philosophy*, 60 (1965), 364-74.

to be true. (2.6) together with (2.5) and the definition (2.1) entails

(2.7) $$(x)[Gx \equiv F_3x]$$

This in turn entails

(2.8) $$(x)[(Gx \,.\, F_3x) \equiv F_3x]$$

If one now defined

(2.9) 'G^*x' is short for '$F_1x \,.\, F_2x \,.\, F_3x$'

one could obtain by deduction from (2.2) and (2.6) the generalisation

(2.10) $$(x)[G^*x \equiv F_1x]$$

(2.10) would be known to be true, since it is validly deduced from premisses known to be true. From (2.3) and (2.6) and using (2.9) one would also deduce

(2.11) $$(x)[G^*x \equiv F_2x]$$

Finally, (2.8) and (2.9) give

(2.12) $$(x)[G^*x \equiv F_3x]$$

(2.10), (2.11), and (2.12) are, of course, all synthetic.

What is noteworthy in this transition is the crucial role of (2.6). The definition (2.9) is possible at any time. Definitions are arbitrary, in the sense that the only condition which logic imposes upon them is that the term defined be eliminable wherever it occurs. So logic permits (2.9) to be laid down at any time. But unless (2.6) were (known to be) true this stipulation would be *pointless*. For it is the fact that (2.6) is (known to be) true that enables one to obtain the law statements (2.10), (2.11) and (2.12). The scientist desires to introduce only significant concepts, that is, to define only such terms as enter into statements of law. Unless (2.10), (2.11) and (2.12) were true, the definition (2.9) would not satisfy the condition of significance the scientist's desires impose on definitions. And (2.10), (2.11) and (2.12) can be affirmed only because (2.6) is (known to be) true. Thus, the discovery of (2.6) is a necessary condition which must be fulfilled before the scientist can define 'G^*x' by (2.9) as a means to his desired end.

Having defined 'G^*x' by means of (2.9), as a consequence of the discovery of (2.6) which renders it significant, the scientist could then abandon the term 'Gx'.[1] For, suppose the laws (2.2), (2.3) and (2.5) alone are those which render 'Gx' significant. These are deducible from (2.10), (2.11) and

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[1] '*Could* abandon' but not necessarily '*would* abandon'. The scientist might have reasons for retaining it. See below.

(2.12). In addition, from these three (2.6) is also deducible. On the other hand, the latter is *not* deducible from (2.2) and (2.3). Other things being equal, it is, clearly, both more elegant and more convenient to express what one knows after the discovery of (2.6) in terms of 'G^*x' rather than 'Gx'. For this reason, if the scientist has no definite reason for retaining 'Gx' in his vocabulary, the definite tendency will be for him to abandon it, letting 'G^*x' replace it.

It must, of course, be emphasised that the gain is one of convenience and elegance only. Factual content is not increased. (How could it be by a mere redefinition of concepts?) Nor can one fail to state (2.10)-(2.12). There is no possibility for the elimination of the law statements; a statement true by definition cannot replace a statement of law. (The concept 'G^*x', since it is a defined *term*, cannot *by itself* or in a statement true by virtue of its definition say anything at all about facts.[1]) However, convenience and elegance count, and when the scientist comes (say) to rewrite his textbook, he will probably add a new indicator, redefine his concepts.

The above is quite schematic. But not, I think, unrealistic in essentials. One point of unrealism however, is crucial. The scientist would not mark his two defined concepts as 'Gx' and 'G^*x'. On the contrary, he would simply *redefine the same mark.* At the end of the process, after the discovery of (2.6), 'Gx' would be defined as short for '$F_1x \,.\, F_2x \,.\, F_3x$'. Logically, this presents no problem whatsoever. Nor is it unreasonable to describe *the process* as one in which new 'indicators' are added to a concept in the light of the discovery of the truth of a justificatory synthetic generality. *However, if one concentrated on this process* (the context of discovery) *rather than on the product of this process*, viz. definition (2.9) (the context of justification), *then one might well be (mis)led into believing that the scientist was employing reduction sentences.*

This point can be made even more evident from the following considerations.

(2.2) and (2.3) entail

$$(2.13) \qquad (x)[F_1x \supset (Gx \equiv F_2x)]^2$$

(2.13) has the logical form of a reduction sentence. (However, it is *not* in fact a reduction sentence. For 'Gx' is explicitly defined by (2.1). If (2.13) were what Carnap and Hempel call a reduction sentence, then 'Gx' would not be a defined predicate.) Furthermore, (2.6) entails

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[1] It has been wrongly held by some, notably Hegel and Dewey, that a statement true by definition can do the job of a statement of fact. For a discussion and critique of the general pattern, see G. Bergmann, *Philosophy of Science* (Madison, Wisc., 1958), pp. 62 ff. and 79 ff. For Dewey in particular, see M. Brodbeck, 'The New Rationalism: Dewey's Theory of Induction', *The Journal of Philosophy*, **46** (1949), 781-91.

[2] (2.13) is entailed by (2.3) alone without the assistance of (2.2). But this is by virtue of the 'peculiarities' of the material conditional alone. The addition of (2.2) established that the connection between F_1 and the other predicates is 'non-accidental', as it were. It provides the 'connection' that the material conditional alone does not provide.

$$(x)[F_1x \supset (F_2x \equiv F_3x)] \tag{2.14}$$

Within the reduction sentence framework, (2.14) would constitute sufficient justification for introducing a second 'reduction sentence':

$$(x)[F_1x \supset (Gx \equiv F_3x)] \tag{2.15}$$

[That is, (2.14) plays the role that is played by (1.3).]

(2.15), too, has the form of a reduction sentence. (Though, again, it too is not literally one.) Furthermore, (2.15) also occurs in the scientist's scheme, being entailed by (2.2) and (2.7).[1] The point is that (2.7), which entails the second 'reduction sentence' (2.15), is affirmable only *after* the discovery of the truth of (2.6); that is, it is affirmable only after the discovery of the truth of the sentence which entails (2.14), where (2.14) is the sentence which *would* justify (2.15) if (2.15) *were* a reduction sentence.

Thus, all the materials for reduction sentences are present. One should not, therefore, be surprised that someone who blurs the distinction between the process of discovery and the products of that process, should end up defending reduction sentences.

Nor is this the only feature of the context of discovery which could lead one to reduction sentences. But before indicating these other features, it should be pointed out that not always upon the discovery of a law like (2.6) does the scientist revise his set of definitions (as from (2.1) to (2.9)).

The question, to state it in terms of our example, is this: Under what conditions would one, and would one not, upon the discovery of (2.6) that 'Gx' and 'F_3x' are coextensive, redefine 'Gx' so that 'F_3x' now occurs in its definiens where previously it did not?

Now, the answer to the first part, about the conditions under which one would redefine the term, has already been answered a few paragraphs back. The answer, roughly, is that one would redefine when he had no motive for not redefining. For, other things being equal, the new concept is more significant than the old. More accurately, it can be used to state the same laws as the old concept, plus additional ones. And, as the scientist's desire is for significant concepts, this makes the new term more appropriate for his end than the old.

On the other hand, things are not always equal.

The basic desire of the scientist is (by definition) the statement of laws. *Secondarily*, he wants these laws to fit together into a *theory*. A theory is a set of laws arranged into an axiomatic system.[2] A law may or may not occupy a crucial place in this structure. To abandon one law as false may require only slight modifications in the structure. To abandon another may have far-reaching effects. Thus, some laws are of great *theoretical significance*; others are of but minor theoretical significance. Scientists, therefore, desire not only significant concepts, but also concepts which enter into laws of great theoretical significance.

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[1] See n. 2, p. 295.

[2] Cf. H. Hochberg, 'Axiomatic Systems, Formalization, and Scientific Theories', in L. Gross (ed.), *Symposium on Sociological Theory* (New York, 1959).

Suppose the laws (2.2) and (2.3) were part of a theory. Suppose further that they are of great theoretical significance. Finally, suppose (2.6) is of little theoretical significance. Since (2.10), (2.11) and (2.12) are true only if (2.6) is true, it follows that these statements, like (2.6), but unlike (2.2) and (2.3), embody knowledge which is of little theoretical significance. In such a case, the scientist will not want his theoretical formulations encumbered by the presence of (2.6) and its consequences. It is (2.2) and (2.3) which are crucial; (2.6) can be fit in later, so to speak, as it is of only peripheral interest. Circumstances being thus, the scientist will have good motivation for not redefining 'Gx' so as to include 'F_3x' in its definiens.

Thus, the answer to the second part of the question asked above, about the conditions under which one would not redefine the term, is this: One would not redefine it upon the discovery of new laws in the case that the new laws are of no theoretical significance, while the laws into which the unredefined concept enters are of some theoretical significance.

The following examples illustrate my point.

Consider the phrase 'x is magnetic'. We have the two definitions:

(2.16) 'x is magnetic$_1$' is short for '(x is near a small iron object) $\supset$ (the object moves toward x)'

(2.17) 'x is magnetic$_2$' is short for '(x is moved through a small iron loop) $\supset$ (an electric current flows in the loop)'[1]

and also the law

(2.18) (x) (x is magnetic$_1$ $\equiv$ x is magnetic$_2$)

This law permits us to introduce the definition

(2.19) 'x is magnetic' is short for '(x is magnetic$_1$) . (x is magnetic$_2$)'

If one is asked for the definition of 'magnetic' it is not possible to indicate just (2.16) or just (2.17). Only (2.19) suffices. For 'magnetic' means *both* what (2.16) says and what (2.17) says. However, the only reason we have the concept 'magnetic' defined just as in (2.19) is because we know the law (2.18). It is (2.18) which establishes that an object's being magnetic_1 is sufficient for its being magnetic. Without (2.18), (2.19) would be logically impeccable but quite useless. And, on the other hand, neither the concept defined by (2.16) nor that defined by (2.17) occurs in laws of greater theoretical significance than the other. For these reasons magnetic_1 and magnetic_2 are both properties the presence of which, either separately noted or jointly, suffice to indicate the presence of the property magnetic. Magnetic_1 and magnetic_2 are coextensive, and occur within the theoretical structure of science at exactly the same points; they are, therefore, equally definitory of what it is to be magnetic.[1]

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[1] Recall that I am assuming that the appropriate sense of 'if-then' for 'operational definitions' is that of the material conditional, supplemented by the condition that the term defined be significant. In this case, it is clear that the significance condition is fulfilled.

In contrast, consider the case of defining the concept of being hungry (for food) in behaviour theory. There are several possibilities:

(2.20) 'x is hungry_1' is short for 'x exhibits stomach contractions'

(2.21) 'x is hungry_2' is short for 'x has not eaten for such and such a length of time'

(2.22) 'x is hungry_3' is short for '(x is presented with food) $\supset$ (x consumes the food)'.

In the case of humans we can add:

(2.23) 'x is hungry_4' is short for '[(x is in such and such a set) . (x is asked "hungry?")] $\supset$ (x utters "I-am-hungry")'

Finally, if we are sophisticated in our physiology, we can supplement (2.20) with

(2.24) 'x is hungry_5' is short for 'x has such and such an amount of sugar in his blood'

We know that all of these concepts are as a matter of fact coextensive. These laws of coextensivity determine that the concept defined by the conjunction of the predicates defined by (2.20)-(2.24) is a significant concept.

The concept defined by the conjunction of the terms defined by (2.21), (2.22), (2.23) and (perhaps) (2.20) is, I believe, a not unfair analysis of our 'ordinary' concept of being hungry. When we say of a person that he is hungry, we are applying to him all the predicates of this conjunction. To 'explain' what it is to be hungry, it is necessary to indicate each of the properties defined by (2.20) through (2.23) and also the fact that the properties thus defined are coextensive.[2]

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[1] This statement is true, of course, only at the phenomenological level. It does not hold for the concept which applies to the entities in the model of the theory of the structure of matter. The 'operationally defined' terms of the phenomenological level are co-ordinated to certain highly defined model terms. None of the latter is ever directly interpreted.

[2] One might note that such 'explanations' are what go by the name of 'conceptual analysis' at Oxford and elsewhere. Conceptual analysis involves noting explicit definitions, on the

However, as more and more knowledge of laws has become available and the outlines of a theoretical structure have become apparent, it has become clear that the concepts which belong to the laws of greater actual and (we expect) potential theoretical significance, are the physiological. That means the concepts defined by (2.20) and (2.24) become of greater importance than those defined by (2.21)-(2.23) as the science of psychology develops. This accounts for the tendency (which is not all that new) to take only the physiologically defined concepts as definitory of 'hungry' while the behaviourally defined concepts, those which occur in laws of lesser theoretical significance, are taken to be merely the 'symptoms' of hunger.

We can now partially answer a point made by Madden[1] against the definitionalist's thesis.

> If one explicitly identifies the meanings of 'If a magnetic needle is put close to the wire, it deflects', or whatever, with 'There is an electric current in this wire,' then he is unable to give as a reason why something is what it is, a synonymous phrase . . . if one asks, 'Why did this needle move?', I have not answered him at all if, having asserted the equivalence of needle moving and electric current, I reply, 'because the wire conducts an electric current.' But this is precisely what I want to do. Whenever one sees a magnetic needle held near a wire move, . . . he can legitimately say by way of explaining these facts, or giving reasons for their occurrence, 'There is an electric current in this wire.' . . . the observational

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one hand, and laws of nature, on the other. To the extent that these are not distinguished, 'conceptual analysis' is simply confused.

Terms conjunctively defined on the basis of a series of other terms which are, as a matter of lawful fact, coextensive, provide (I believe) a reasonable explication of the following remarks of Sellars: '. . . while a term cannot have several explicit definitions without being ambiguous, a term may have several sufficient minimal criteria without being ambiguous, provided that the criteria are satisfied by the same objects. (And even where they are not. This would not mean that the term was *ambiguous* [his italics], though it would mean that revision of its criteria was called for.)' (W. Sellars, 'Comments on Maxwell's "Meaning Postulates in Scientific Theories" ', in H. Feigl and G. Maxwell (ed.), *Current Issues in the Philosophy of Science* (New York, 1961), p. 188.)

It also explicates, I believe, such notions as Strawson's 'P-predicates'. But to argue that would take me beyond the purposes of the present paper.

I might note, finally, that what is said in the present paper with respect to conjunctively defined concepts holds only in the case that both test procedures (both conjuncts) apply to all objects in the relevant universe of discourse. This restriction means what is said does not apply to, e.g. temperature, defined once in terms of a mercury thermometer, once in terms of a platinum thermometer. The latter can measure the temperature of objects for which the use of a mercury thermometer is physically impossible; this being so, the two concepts are not, as a matter of fact, coextensive. However, it is possible to handle this case by defining temperature as the disjunction of the two test procedures. This requires, of course, that the two tests yield the same result in those cases where they both apply, that for these cases they be coextensive.

The connection between the conjunctive and disjunctive cases can be seen by noting that, if G is coextensive with H, then $G \vee H$ is coextensive with $G \, . \, H$, and each of these complex predicates is coextensive with each of G and H.

[1] E. H. Madden, 'Definition and Reduction', *Philosophy of Science*, 28 (1961), 390-405.

evidence is explained, in part, at any rate, by the theoretical terms to which it is related and hence could not be an explicit definition of it but rather a criterion for its correct use.[1]

Let us write '*x* is electric' as short for 'there is an electric current flowing in *x*'. This is merely for our own convenience.

Madden mentions the definition

(2.25) 'x is electric$_1$' is short for '(x is put close to a magnetic needle) $\supset$ (the needle deflects)'

This shows that he is concerned, primarily, with the phenomenological level, the level of 'operational definitions'. What must be noted is that (2.25) gives only *part of* the meaning of the operationally defined concept '*x* is electric'. We also have, for example,

(2.26) 'x is electric$_2$' is short for '(a resistance coil is connected into x) $\supset$ (the coil heats up)'

and

(2.27) 'x is electric$_3$' is short for '(x is put in such and such a solution) $\supset$ (so and so precipitates)'

Certain true generalities establish that the concepts defined by (2.25), (2.26) and (2.27) are coextensive. These laws justify the definition of '*x* is electric' as the conjunction of the predicates defined by (2.25)-(2.26). For, it is these laws which guarantee that the thus defined '*x* is electric' is a significant concept.[2]

Now, *I* may stipulate a special meaning for '*x* is electric'. That is my privilege, if I choose to exercise it. I may stipulate, for example, with Madden, that '*x* is electric' is synonymous with 'x is electric$_1$', where the latter is as defined by (2.25). Then Madden's comment is surely not beside the point: I cannot appeal to something's being electric to explain why, when a magnetic needle is put close to it, the needle deflects.[3] But the definitionalist's is not a thesis about *his own* concepts, but about *the scientist's*. And the scientist's concept of 'electric' involves (2.26) and (2.27). And, moreover, it involves these by virtue of certain laws.

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[1] E. H. Madden, 'Definition and Reduction', pp. 398-9.

[2] (2.25) contains 'magnetic' in the definiens. Similarly (2.17) contains 'electric' in its definiens. 'Electric' and 'magnetic' enter in each other's definition. This shows that as one moves up the definitional chains away from the observational base concepts become definitionally interconnected. There come to exist 'conceptual connections' among the more highly defined concepts. But, because there are definitional connections, they are absolutely trivial. What *is* important are the laws among the less highly defined concepts. These laws render the more highly defined terms significant, and justify their introduction.

[3] 'Not beside the point' would perhaps be better as 'not completely beside the point'. I shall suggest below that even here Madden misses the mark to a certain extent.

As Madden says, we want to be able to explain the deflection of the magnetic needle by pointing to the fact that the wire is electric. We want to be able to do this because this is what the scientist does. The piece of commonsense, that this practice of the scientist is (at times) justified,[1] must be accounted for by the definitionalist. If he cannot, then his thesis is inadequate, just as Madden suggests. But, Madden notwithstanding, the definitionalist *can* account for this piece of commonsense.

He merely points out that to say something is electric is to say more than that it is electric$_1$: it is to say also that it is electric$_2$ and that it is electric$_3$; and *moreover*, since the concept of being electric is appealed to by the *scientist*, who, by definition, uses none but significant concepts, it is also to refer (implicitly, at least, or, as one might put it, enthymematically) to certain laws, namely, those which justify the scientist's use of the concept electric. That is, when the scientist explains by citing a concept there is also an implicit appeal to certain laws; in the present example, it could be to (at least) those laws which state that electric$_1$, electric$_2$ and electric$_3$ are coextensive concepts. There is an implicit appeal to those laws which show that the predicates occurring in the definiens of concept cited, are not brought together by mere whim, but are, rather, lawfully connected.

The concept of being electric is defined, to be sure. But it is *significant*. It is this fact of significance which provides the explanatory power behind the scientist's citing the electric current as a reason for the deflection of the needle.

I also want to say that even Madden's objection to the use of electric$_1$ as explanatory is not completely correct. Electric$_1$, even as defined by (2.25), provides some explanation of why the needle deflects.

If the concept defined by (2.25) is to be of interest to the scientist, then (2.25) cannot be an arbitrary stipulation; rather, it must be a stipulation defining a significant concept, and, to that extent, non-arbitrary. That means that in addition to the linguistic connection between antecedent and consequent of the definiens in (2.25) which the stipulation (2.25) establishes, there is also a *lawful* connection. These laws provide a non-linguistic connection which justifies defining the concept electric$_1$. Citing the wire's being electric$_1$ as an explanation of the needle's deflection is explanatory in so far as there is an implicit reference to this non-arbitrary, significant connection among concepts which serves as justification for the stipulation (2.25).

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[1] This point can be much generalised. Much has been made recently of historical explanations in which none but a concept is mentioned. This does not affect the 'covering law model' of explanation. The defender of that 'model' needs merely to point to the laws which make the concept appealed to a significant concept; that is, point to the laws which account for the presence of that concept in our vocabulary. Of course, the 'laws' may be of a very imperfect sort, and even false but believed for ideological reasons. That is not the point. The point is that these laws (or what are believed to be laws) give the concept its explanatory power (what is felt to be its explanatory power). See M. Brodbeck, 'Explanation, Prediction, and "Imperfect" Knowledge', in H. Feigl and G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science*, vol. iii (Minneapolis, 1962).

Not that this explanatory power is very great. It is not. But it is there nonetheless. It provides, I suggest, the non-anthropomorphic and common-sense core of the scholastic and Rylean idea that citing a disposition serves to explain why certain objects do certain things in certain circumstances.[1] And, I should point out, such explanatory power increases as one moves up the definitional chains to the more highly defined concepts of the hierarchy. Pointing to something's being electric has more explanatory power than pointing to its being electric$_1$. For, more generalities must be true before 'electric' is significant than before 'electric$_1$' is significant.

Thus far, we have stayed, as Madden suggests, at the level of 'operational definitions'. But we might also note that within the model of the theory of the structure of matter, one finds yet another definition of the concept electric. Call this concept 'electric$_m$'. This is defined within the model in terms of electron flow, etc. 'Electric$_m$' is not operationally defined; it does not occur at the phenomenological level. The theory does, however, co-ordinate this concept to that of 'electric', as operationally defined. This co-ordination is such that there is an electric$_m$ current flowing if an only if there is an electric current flowing.[2] This accounts for the fact that, as science has grown, concepts of the model have come to be part of the definition (meaning) of electric. This is analogous to the trend to include physiological factors as part of the definition of hungry. And, indeed, for the practicing physicist, electric$_m$ becomes the defining characteristic of being electric. It does this by virtue of fact that electric$_m$ occurs in laws of high theoretical significance within the total structure of physics. Electric$_1$, electric$_2$, and electric$_3$ are quite peripheral compared to electric$_m$.

It is, moreover, this high theoretical significance of the laws in which it occurs that gives the citation of electric$_m$ its high explanatory power.

This, however, is a digression (though it helps to explain why Madden's points should seem *prima facie* plausible). Return to the phenomenological level; to the 'electric' operationally defined as the conjunction of the predicates defined by (2.25)-(2.27).

I agree with Madden that 'the observational evidence is explained, in part, at any rate, by the theoretical terms to which it is related'. That is, one can explain why a wire deflects a magnetic needle by pointing to the fact that the wire is electric (or even: electric$_1$). But, I say against Madden, it does not follow that 'hence [the observational evidence] could not be an explicit definition of [the theoretical term]'. This follows only if the sole condition a definition needs to fulfil is that it be eliminable. If this were the sole condition then the theoretical term would be a *mere* abbreviation of the observational evidence, and so no explanation of it. To be sure, eliminability is the only condition that *logic* imposes. But the scientist *desires* to define only *significant* concepts. His desires impose a further condition besides eliminability: significance. This further condition insures that any theoretical term which is acceptable to the scientist, even though it may be explicitly defined in terms of the observational evidence, is not a mere abbreviation of the latter. And this fact that it is not a *mere* abbreviation, that it is also significant, accounts for the explanatory power of scientifically acceptable defined terms.

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[1] For a discussion of Ryle, see the study of L. Addis, 'Ryle's Ontology of Mind', in L. Addis and D. Lewis, *Moore and Ryle: Two Ontologists* (The Hague, 1965).

[2] Cf. G. Bergmann, 'Reduction', in *Current Trends in Psychology and the Behavioural Sciences* (Pittsburgh, 1955).

(*to be concluded*)

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Relevance and the Ravens*

by C. A. HOOKER and D. STOVE

I

Let us consider:

(1) Non-black non-ravens do not confirm 'All ravens are black'

and

(2) Non-black non-ravens do confirm 'All non-black things are non-ravens'.

These are plausible statements of confirmation. But there is a general principle of confirmation, viz.:

(3) Whatever confirms a given hypothesis also confirms any statement logically equivalent to it

which is at least as plausible, but inconsistent with them. This fact constitutes the problem about the ravens.

The most popular solution to this problem has been along the following lines: that statement (1) is false; and that its plausibility is to be explained by the fact that, the world being as it actually is, evidence consisting of non-black non-ravens is more likely to be met with, and hence confirms 'All ravens are black' *less*, than the 'standard' evidence for it, viz. that which consists of black ravens.

This attractive idea has been developed by a number of writers since it was first advanced.[1] The most clear and explicit version of it is contained in an admirable recent article by Professor J. L. Mackie,[2] and it is this version of the solution which is the subject of the present paper.

The solution, as Professor Mackie presents it, rests on two groups of premises. First, on a certain criterion of confirmation, and a certain criterion of comparative confirmation, viz.:

(A) p confirms q in relation to r if and only if the logical probability of p relative to $(r \,.\, q)$ exceeds that of p relative to r.[3] (We abbreviate this condition, '$Pr(p, r \,.\, q) > Pr(p, r)$'.) Similarly, p disconfirms (is neutral to) q in relation to r if and only if $Pr(p, r \,.\, q) < (=)\ Pr(p, r)$.

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* Received 27.vi.67

[1] By Janina Hosiasson-Lindenbaum, 'On Confirmation', *Journal of Symbolic Logic*, 5 (1940), 133.

[2] 'The Paradox of Confirmation', this *Journal*, 13 (1963), 265.

[3] This may alternatively be stated in a non-relativised form, as: p confirms q if and only if $Pr(p, q)$ exceeds the 'absolute' probability $Pr(p)$.

(B) p_1 confirms q in relation to r more than p_2 does, if and only if

$$\frac{Pr(p_1, r \,.\, q)}{Pr(p_1, r)} > \frac{Pr(p_2, r \,.\, q)}{Pr(p_2, r)}.$$

Second, the solution rests on—what these two principles obviously must be supplemented by, if they are ever to be put to work—certain numerical statements of logical probability. More specifically, what is evidently needed are two assessments of the logical probability of evidence of the black raven sort; first relative to the almost irresistible commonsense assumptions about the commonness of ravens and of black things; second, relative to this information conjoined with the hypothesis that all ravens are black. Then we will need two corresponding assessments of the probability of evidence of the non-black non-raven sort. Before we give the four assessments of these probabilities on which the solution has been made to rest, certain abbreviations need to be introduced.

Let us write 'k' for the statement, which may fairly be taken to express commonsense belief: 'a proportion x, $1>x>0$, of all things are ravens; a proportion y, $1>y>0$, of all things are black; and $x<y<\frac{1}{2}$'. We write 'h' for: 'All ravens are black'; 'b_1', or '$Ra \,.\, Ba$', for the statement: 'The individual a is a raven and a is black'; 'b_4' or '$\sim Ba \,.\, \sim Ra$' for 'a is not black and a is not a raven'; and 'b_2' and 'b_3' respectively for the statements 'a is a raven and a is not black' and 'a is not a raven and a is black'.

The four probabilities referred to above are now asserted to have the following values: $Pr(b_1, k) = xy$; $Pr(b_1, k \,.\, h) = x$; $Pr(b_4, k) = (1-y)(1-x)$; $Pr(b_4, k \,.\, h) = 1-y$.

The assumptions[1] necessary to justify these assessments also suffice to determine the values of the probabilities of both b_2 and b_3, both relative to k and relative to $(k \,.\, h)$. The eight probabilities thus fixed are conveniently represented in the following table.

Probability of	b_1	b_2	b_3	b_4
relative to k	xy	$x(1-y)$	$y(1-x)$	$(1-y)(1-x)$
relative to $(k \,.\, h)$	x	0	$y-x$	$1-y$

Using only the premises we have stated, the solution now proceeds as follows. Since $x>xy$, it follows by the criterion of confirmation (A) that b_1 confirms h, in relation to k. But so too does b_4, by (A), since $(1-y)>(1-y)(1-x)$. So the statement (1) above was false—non-black non-ravens do confirm 'All ravens are black'—and the deadlock is broken. However, since $\frac{x}{xy} > \frac{1-y}{(1-y)(1-x)}$, it also follows by the principle of comparative confirmation (B), that h is confirmed *less* by b_4 than by the 'standard' evidence b_1, in relation to information (k) which it is almost impossible not to be aware of. The plausibility of (1) is thus accounted for without its truth being conceded; the error of taking less confirmation for absence of confirmation being one to which intuition is no doubt prone.

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[1] What these are will appear in Section III below.

We will try to show in what follows, first, that this is an unacceptable solution to the problem of the ravens, because it entails one or more paradoxical statements of confirmation; and second, that the solution does not suffer from the same defect when it is applied to a problem often mistakenly identified with the problem of the ravens.

It should be noticed, however, that an unacceptable solution to the problem—the one just stated, for example, if we are right—might happen, notwithstanding, to be a 'descriptively correct' solution. That is, the premises on which it rests might all be ones we do or would accept *in fact*; and any 'errors', by way of departure from the consequences of those premises, which the solution has to ascribe to us, might be errors to which we are in fact prone. The possibility that the solution stated above is descriptively correct will not be touched on here. Our main object is to show that, whatever the facts may be, this solution is not one we *ought* to accept.

II

The table of logical probability-values reproduced above, while of course it satisfies (when taken with (A)) the obvious desideratum that b_2 disconfirms h, was prized chiefly for producing all the inequalities, both of probabilities and of quotients, which the solution requires in columns one and four. The values in column three, in consequence, were fixed only accidentally, so to speak, and subsequently neglected. Yet in fact they embody a paradox which, if it cannot be avoided, suffices to discredit the premises on which the solution rests.

For since $y-x<y(1-x)$, it follows in virtue of (A) that relative to k, b_3 disconfirms h. That is, that '$\sim Ra\,.\,Ba$'—the individual a might thus be a black boot, for example—*disconfirms* 'All ravens are black'[1]! Or, what is

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[1] Mr H. G. Alexander, with whom the table originated, observed (this *Journal*, 9 (1958), 233) that there was a 'puzzle' about b_3. We have not succeeded in understanding the sentences in which Mr Alexander tried to resolve the puzzle; but in any case he seems to have misconceived the problem. For the puzzle, he says, is that b_3 is 'not confirmatory', which along with his remark that b_1 and b_4 would always be 'better evidence' than b_3, suggests that he may not have realised that b_3 according to his table is actually *disconfirmatory*.

Professor Mackie did notice (p. 267 of the article referred to in note 2, p. 305) that, relative to k, b_3 disconfirms h. He adds, '. . . but only slightly'. This addition is certainly less than would be needed to soften the blow. It is also more than Professor Mackie was entitled to say; for his premises certainly did not include any criterion of *degree* of confirmation.

equivalent according to (A),[1] that this hypothesis is *confirmed* by '$Ra \vee \sim Ba$'!

III

If this consequence, that black non-ravens disconfirm 'All ravens are black',[2] is to be avoided, it must be by the denial of (A), or of some premise used in arriving at the values in column three of the table. But just what were the premises so used? To answer this question we must make the solution under discussion even more explicit than Professor Mackie made it.

To arrive at any of the statements set out in the table, it is of course necessary to assume certain general principles of probability; for example the negation and conjunction axioms. But these are hardly likely to be controverted, so there is no need to enumerate which premises of this kind lie behind the table. The axioms and theorems of probability serve only to make possible the derivation of some *statements* of probability *from other such statements*. The statements made in column three are: $Pr(\sim Ra \,.\, Ba, k) = y(1-x)$, and $Pr(\sim Ra \,.\, Ba, k \,.\, h) = y-x$. What we seek, then, are the statements of probability from which these are derived.

The following set provides the answer, for the statements (*a*)-(*f*) are necessary (logical equivalents counting as identical, of course), as well as sufficient, to yield the values of the probabilities as they are stated in column three:

(*a*) $Pr(Ra, k) = x$
(*b*) $Pr(Ba, k) = y$
(*c*) $Pr(Ba, k \,.\, Ra) = Pr(Ba, k)$
(*d*) $Pr(Ra, k \,.\, h) = Pr(Ra, k)$
(*e*) $Pr(Ba, k \,.\, h) = Pr(Ba, k)$
(*f*) $Pr(Ba, k \,.\, h \,.\, Ra) = 1$.

The same set is also necessary and sufficient to justify the probability-statements asserted in columns one, two and four.[3]

Consequently, if we are to avoid the paradox we found above, it must be by denying either (A) or one at least of (*a*)-(*f*).

IV

Let us first consider denying (A)—denying, that is, the thesis that p confirms q in relation to r if and only if $Pr(p, r \,.\, q) > Pr(p, r)$. Professor

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[1] See n. 3 p. 311 below (the second paragraph).

[2] Here, as at some other places, we follow a common usage, and do not distinguish between a hypothesis being confirmed (or disconfirmed, etc.) by a certain *statement*, and by certain *objects*. There is no sanction for this usage in the criterion of confirmation (A), which clearly requires the evidence to be a statement and nothing else; and the usage is probably indefensible. (Cf. D. Stove, 'Hempel's Paradox', *Dialogue* 4 (1966), p. 446.) But since nothing in the present paper depends either on this usage or on its avoidance, we have sometimes followed it and sometimes not.

[3] Not quite all of (*a*)-(*f*) are necessary for any column other than the third; columns one and two do not need (*e*), and column four does not need (*d*).

Mackie called this, 'the Inverse Principle', but we will call it instead 'the Relevance Criterion of Confirmation', a name we prefer for two reasons. First, it imports, what is evidently true, that what the thesis proposes is a criterion of *confirmation*; whereas Professor Mackie's name both omits this truth and suggests,[1] what is clearly false, that the thesis is a principle of *probability*. Second, our name invites attention to the historical background of the thesis, which is very instructive, if the following account of it is correct.

The theory of probability must always have recognised, at least tacitly, that statements of probability are of at least two kinds. Some are numerical, that is, they assert that the value of a certain probability is equal to (or greater, or less, than) a certain number. (For example, the statements (*a*), (*b*) and (*f*) above.) Others are not numerical, but comparative, that is, they say of a certain probability only that its value is equal to (or greater, or less, than) that of a certain other probability. But in J. M. Keynes's epoch-making contribution to the theory of probability,[2] comparative statements of probability were accorded a much more prominent place than they had ever occupied in the earlier history of the subject.[3] As a natural consequence, Keynes was led to distinguish different kinds of comparative statement, and to coin names for some of these kinds.

One such kind consists of those statements which assert the equality of two probabilities, the 'primary propositions'[4] of which differ only in that the evidence-statement in one is a conjunction, one conjunct of which is the whole of the evidence in the other: statements, that is, of the kind '$Pr(p, r \,.\, q) = Pr(p, r)$'. Keynes called these, statements ('judgments') of 'irrelevance'. Similarly he said that '$Pr(p, r \,.\, q) > Pr(p, r)$' asserts the 'favourable relevance' (and the opposite inequality the 'unfavourable relevance') of q to p, in relation to r.[5]

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[1] Because of the long-standing and well-known application of the word 'inverse' to a certain part of the theory of probability. The usage of the phrase 'inverse probability' has been confused enough—what is so called being sometimes certain *theorems* of probability, sometimes certain *statements* of probability—without adding to the confusion by allowing a similar phrase to become current for a certain criterion of *confirmation*.

[2] *A Treatise on Probability*, 1921.

[3] Op. cit. Part I, especially chapter III.

[4] Op. cit. p. 11 ff.

[5] Actually, Keynes's usage was seriously variable, and defective. Of '$Pr(p, r \,.\, q) = Pr(p, r)$', he would indeed sometimes say that it asserted the irrelevance of q to p on r; i.e. he sometimes speaks in the way we have ascribed to him, which is essentially also Carnap's way, and the one we follow. But more often he says that it asserts the irrelevance of q *to the probability of* p, on r. Both of these usages may be found, for example, on pp. 54-55 of the *Treatise*. But most often of all he describes it in yet a third way, as asserting the irrelevance of q *to the argument* p,r. This is a most anomalous usage, because 'p,r' is Keynes's abbreviation, not for the argument from r to p, but for the *probability of* the argument from r to p.

These coinages of Keynes, although they were nothing more than that, have facilitated the making of two remarkable identifications, distinct though connected, and both in our opinion mistaken.

First, they have facilitated tacit identification, by some philosophers,[1] of what Keynes called 'irrelevance' with 'irrelevance' in its ordinary sense (whatever that may be). This identification needs only to be made explicit, for its falsity to be evident.

Second, and much more important, they have facilitated the widespread identification—apparently made by Keynes himself[2]—of what Keynes called 'favourable relevance' with *confirmation*; or if not their identification, at least the opinion that the favourable relevance of q to p is an adequate criterion of the confirmation of q by p. Now we do not deny that the Relevance Criterion of Confirmation is a very plausible thesis. It is not more so, however, than many other theses which philosophers treat with deep distrust, and for the acceptance of which they demand arguments. What is remarkable in the present case is that, without anyone ever having been obliged to advance arguments for it, the Relevance Criterion has acquired so unique an authority that it is treated as a settled fact, even by two great philosophers who are in other opinions opposed, and even when it is inconsistent with the other opinions of each.

Thus, Carnap's acceptance of the Relevance Criterion[3] seems to be so implicit as to indicate that, for him, favourable relevance is not so much a criterion of confirmation as confirmation itself.[4] Yet this identification is inconsistent with the other identification, which Carnap also made,[5] of degree of logical probability $Pr(p, q)$ with the degree to which q confirms p. It was Popper who showed this.[6] Yet Popper himself accepts the Relevance Criterion,[7] although it is conspicuously open to one of the very criticisms—that which concerns the 'weight' of evidence—which Popper regards as fatal when he advances it against theories of confirmation other than his own.[1] For consider the next throw with a homogeneous die, and let 'a' be the statement that 6 will not turn up; let 'b' be the statement that in the last 600,000 throws this die has turned up 6 only $100{,}000 \pm 10$ times. Then intuitively, no doubt, b confirms a. The initial probability of a, Popper tells us,[2] is $= \frac{5}{6}$. But relative to the 'ideal evidence' b, it must surely be the same, i.e. $Pr(a, b) = \frac{5}{6}$; whence b is—in, of course, the technical sense—irrelevant to a, and hence by the symmetry[3] of irrelevance, does not confirm it according to the Relevance Criterion.

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[1] E.g. Popper, *The Logic of Scientific Discovery*, p. 408. Here Popper reaches a conclusion which is paradoxical, as he supposes it to be, only if 'irrelevant' is taken in the everyday sense; from premises which establish only 'irrelevance' in the technical sense. Again, Carnap uses the same word in the two senses, in close juxtaposition, without distinguishing them. E.g. *Logical Foundations of Probability*, pp. 474-5.

[2] Cf. *Treatise*, p. 147, the comment on theorem (32).

[3] Op. cit. pp. 463-4.

[4] Consequently Carnap seems unable to bring himself to treat Hempel's definition of confirmation as being, what it evidently is, (for certain cases where the background information is null), an *alternative* criterion of confirmation to the Relevance Criterion. As a result, he begs many questions about confirmation. For example, in order to show, against Hempel, that what confirms a hypothesis need not confirm its consequences, he thinks nothing more is needed than for him to show that *favourable relevance* does not obey such a 'consequence condition'. Cf. op. cit. p. 474.

[5] In the first edition of *Logical Foundations of Probability*, though not in the second. See the preface to the latter, sections B and C.

[6] In a note reprinted in Appendix* ix of *The Logic of Scientific Discovery*.

[7] Op. cit. p. 389. It is stated here in the nonrelativised form mentioned in n. 3, p. 305 above.

Arguments for accepting the Relevance Criterion are needed, then, but are nowhere to be found, as far as we know; while where it has been accepted without arguments, it has displayed to a marked degree the habits of the young cuckoo. It must be admitted, therefore, that one possible way to avoid the paradox we found above would be simply to abandon premise (A).

To do so, however, would be to dismantle altogether the solution we are discussing; for although the part played in it by the Relevance Criterion was not made clear by any writer before Professor Mackie, it is obvious now that it was an essential premise all along.

We must, therefore, turn our attention to the other premises which contributed to the result that b_3 disconfirms h.

V

To avoid this result we must deny at least one of the statements of probability (*a*)-(*f*). It is out of the question to deny (*f*); so the possibilities are narrowed to (*a*)-(*e*).

They could be narrowed further, if the feature of column three which we are trying to avoid were a logical consequence of some proper subset of (*a*)-(*e*), in conjunction with some feature of the table which the solution requires to be preserved. Such is in fact the case. Unfavourable relevance in column two is clearly a feature of the table essential to the solution;

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[1] Op. cit. pp. 407-8.

[2] Op. cit. p. 390.

[3] It is obvious from the conjunction axiom of probability that unless $Pr(p, r)$ or $Pr(q, r)$ is zero (a case of no interest from the point of view of confirmation), $Pr(p, r . q) = Pr(p, r)$ if and only if $Pr(q, r . p) = Pr(q, r)$. Favourable and unfavourable relevance are similarly symmetrical, and hence, according to the Relevance Criterion, confirmation and disconfirmation are each symmetrical.

Other theorems of probability, stating obvious properties of relevance, which are assumed at several points in this paper, are the following: $Pr(p, r . q) > Pr(p, r)$ if and only if $Pr(\sim p, r . q) < Pr(\sim p, r)$; $Pr(p, r . q) < Pr(p, r)$ if and only if $Pr(\sim p, r . q) > Pr(\sim p, r)$; $Pr(p, r . q) = Pr(p, r)$ if and only if $Pr(\sim p, r . q) = Pr(\sim p, r)$. These are obvious consequences of the negation axiom. From them, by the Relevance Criterion (relative to r), p confirms (disconfirms) q if and only if not-p disconfirms (confirms) q, and p is neutral to q if and only if not-p is neutral.

yet it entails the paradox of unfavourable relevance in column three, given only the premises (*d*) and (*e*).[1] Hence avoidance of the paradox requires denial of (*d*) or of (*e*).

Denial of (*d*) or (*e*) requires affirmation[2] of one of the following exclusive alternatives, that to *h*, relative to *k*:

'*Ra*' is unfavourably relevant and '*Ba*' is
1. irrelevant
2. favourably relevant
3. unfavourably relevant

'*Ra*' is favourably relevant and '*Ba*' is
4. irrelevant
5. favourably relevant
6. unfavourably relevant

'*Ra*' is irrelevant and '*Ba*' is
7. favourably relevant
8. unfavourably relevant

While we wish to avoid the paradox in column three, we are under other constraints besides that of preserving unfavourable relevance in column two; the solution we are trying to repair also requires favourable relevance in columns one and four. Now, it can be shown that only one of the above eight possibilities is consistent with all these desiderata being met.

Suppose we say that '*Ba*' is unfavourably relevant to *h*, relative to *k*. Avoidance of unfavourable relevance in column three is equivalent to $Pr(Ba, k . h) \times Pr(\sim Ra, k . h . Ba) > Pr(Ba, k) \times Pr(\sim Ra, k . Ba)$, which under the supposition entails that $Pr(\sim Ra, k . h . Ba) > Pr(\sim Ra, k . Ba)$. But preservation of favourable relevance in column one requires, under the same supposition, that $Pr(Ra, k . h . Ba) > Pr(Ra, k . Ba)$, i.e. that $Pr(\sim Ra, k . h . Ba) < Pr(\sim Ra, k . Ba)$. Thus the requirements for columns one and three conflict under this supposition. This excludes possibilities 3, 6 and 8. A similar argument will suffice to exclude all the rest, except 2.

Suppose '*Ra*' is favourably relevant to *h*. Then the favourable relevance we require in column four in turn requires that $Pr(\sim Ba, k . h . \sim Ra) > Pr(\sim Ba, k . \sim Ra)$, while avoidance of unfavourable relevance in column three requires that $Pr(\sim Ba, k . h . \sim Ra) < Pr(\sim Ba, k . \sim Ra)$. This excludes 4 and 5. Suppose '*Ba*' is irrelevant. Then the solution requires in column one that $Pr(Ra, k . h . Ba) > Pr(Ra, k . Ba)$, while avoidance of paradox in column three requires that $Pr(Ra, k . h . Ba) < Pr(Ra, k . Ba)$.

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[1] In virtue of the theorem of probability that if $Pr(p_1, r . q) = Pr(p_1, r)$ and $Pr(p_2, r . q) = Pr(p_2, r)$ and $Pr(p_1 . \sim p_2, r . q) < Pr(p_1 . \sim p_2, r)$ then $Pr(\sim p_1 . p_2, r . q) < Pr(\sim p_1 . p_2, r)$. Proof of this is obvious if the third clause of the antecedent is expanded in accordance with the negation and disjunction axioms.

[2] We assume that $Pr(p, r . q)$ and $Pr(p, r)$ must be comparable. Keynes (cf., e.g. *Treatise*, p. 65) might have denied this; no one else is likely to do so.

This excludes 1. Finally, if '*Ra*' is irrelevant, we require in column four that $Pr(\sim Ba, k \,.\, h \,.\, \sim Ra) > Pr(\sim Ba, k \,.\, \sim Ra)$, while absence of paradox in column three requires the reverse. This excludes 7.

Consequently, if we are to preserve the elements of the solution under discussion, while avoiding the paradoxical result about b_3, we must affirm the second of the eight possibilities listed above; that is, we must deny both (*d*) and (*e*), and must in fact affirm that, relative to *k*, '*Ra*' is unfavourably relevant, and '*Ba*' favourably relevant, to *h*.

But this is disastrous. For it means, according to the Relevance Criterion, that we must affirm that (relative to *k*) '*Ra*'—or an object characterised just as a raven—disconfirms, and '*Ba*'—or an object characterised just as black—confirms, 'All ravens are black'!

The solution we have discussed is therefore beyond repair, since to preserve it, freed from its first paradoxical consequence, would entail this violent double new paradox.

VI

Let us now consider, instead of our original (1) and (2), the following:
(1′) Non-black non-ravens do not confirm '$(x)(\text{Raven } x \supset \text{Black } x)$';
(2′) Non-black non-ravens do confirm '$(x)(\sim\text{Black } x \supset \sim\text{Raven } x)$'.
These statements of confirmation are plausible enough for the inconsistency of their conjunction with (3) to constitute a problem. But as the problem they constitute has hardly ever been distinguished from that constituted by (1), (2) and (3), it will be worthwhile to show that the solution which we have found wanting when applied to the latter problem, does not suffer from the same defect when applied to the present one.

It needs to be emphasised first, however, that the two problems are distinct.[1] They would be the same only if the hypothesis in (1′), '$(x)(\text{Raven } x \supset \text{Black } x)$'— which we will abbreviate as 'h''—were the same as the hypothesis *h* in (1), i.e. 'All ravens are black'; or at least, were logically equivalent to it. But that is certainly not the case. For example, that there exist neither ravens nor black things, is of course a sufficient truth-condition of h'; but it would be false and even ridiculous to say the same of *h*.

It seems to us, however, that even when *h* and h' are recognised as different, the nature of the difference is often misrepresented, at least by implication. The difference is certainly not, in particular, that the *sentence All ravens are black* is a sentence of a natural language while the *sentence* $(x)\,(Raven\ x \supset Black\ x)$ is one of a certain 'formal language'. There is no bar whatever to the assertion of h' by means of an English sentence; for example, by: *That an individual is a raven materially implies that it is black.* This contains, to be sure, a technicality, 'materially implies', which has to be explained before it is understood. But the explaining can easily be done by saying that the last-mentioned sentence is used to make the same statement as is the quite non-technical English sentence: *Every individual thing is either not a raven, or black.* This is in fact not only a possible, but *the standard,* way of explaining the sentence $(x)(\mathit{Raven}\ x \supset \mathit{Black}\ x)$. Consequently, the *statement* h', if it is understood at all, is understood as being logically equivalent to the *statement,* 'Every individual thing is either not a raven, or black'.

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[1] Cf. the article referred to in n. 2, p. 308 above, pp. 449-53.

But there can be no difficulty about the difference between *this* statement, and h—'All ravens are black'. They are, evidently, very different statements, and even statements of widely different kinds: h being a statement of a kind extremely common in the expression both of scientific and everyday belief; the other being a statement of a most unusual kind. But that is all there is to their difference.[1] Both are perfectly intelligible. We have no difficulty, with respect either to h or to h', in determining intuitively its logical relation to many other statements; or again, in determining intuitively in many cases what does and what does not confirm it.

One statement of confirmation about the hypothesis h' which clearly commands intuitive assent, is that, relative to k, b_3 does not disconfirm it. For b_3 stands to h' as does an object, characterised as possessing both non-ravenhood and blackness, to a hypothesis logically equivalent to the statement that every individual thing possesses at least one of these properties.

Assuming then that one wished to apply to the present problem the same solution as was proposed for the earlier one, it would therefore be more than ever necessary to avoid the first horn of the dilemma on which that solution was there caught. That is, it is imperative to ensure that $Pr(b_3, k \,.\, h') > Pr(b_3, k)$. But our premises, except that h' replaces h in (d), (e) and (f) to give (d'), (e') and (f'), will be the same as before. Now we know that this intuitive requirement on column three can be reconciled with the requirements of the solution on the remaining columns, only if (d') and (e') are denied; and in fact only if they are replaced by

(d″) $Pr(Ra, k \,.\, h') < Pr(Ra, k)$, and
(e″) $Pr(Ba, k \,.\, h') > Pr(Ba, k)$.

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[1] In moving from the first problem to the present one, there is of course no difference, or even the appearance of difference, in the *evidence,* as against the hypotheses. We use, throughout, multiply abbreviated *sentences,* of course, such as $\sim Ra.Ba$; but no one could mistake the statements involved for anything but what they are, viz. statements of a perfectly familiar kind.

But, whereas when the hypothesis was h, the statements corresponding to these constituted (given the Relevance Criterion) a violent double paradox, (d'') and (e'') represent, on the contrary, a perfectly acceptable consequence. Given the Relevance Criterion they say, in fact, no more than intuition demands: that a hypothesis equivalent to 'Every individual thing is either not a raven, or black' is confirmed both by '$\sim Ra$' and by 'Ba'.

It remains to be shown only that a corrected result in column three is consistent with the desiderata for the other columns, now that the second group of premises on which the solution rests are the statements of probability (a), (b), (c), (d''), (e'') and (f').

From (d'') and (a) it follows that $Pr(Ra, k\,.\,h') = x-\phi$, where $x>\phi>0$; and from (e'') and (b), that $Pr(Ba, k\,.\,h') = y+\theta$, where $1-y>\theta>0$. Whence the requirements of the solution on columns one, two and four can be expressed as respectively: $x-\phi>xy$; $x(1-y)>0$; $1-y-\theta>(1-y)(1-x)$; and the requirement for avoiding the paradox in column three as : $y-x+\phi+\theta>y(1-x)$. We want the joint satisfaction of these requirements, or in other words we want: $2x(1-y)>\phi+\theta>x(1-y)>0$. The question whether we can consistently have what we want, is the question whether, for values of x and y within the limits prescribed by k, there are values of ϕ and θ within the limits prescribed above, which make this compound inequality true. There are. For example let $x = 0{\cdot}2$, $y = 0{\cdot}4$, $\phi = \theta = 0{\cdot}1$.

The same example serves also to verify that the inequality of the quotients of columns one and four, which the solution requires, can be preserved at the same time.

Thus, when it was applied to the problem about the confirmation of 'All ravens are black', the solution we have discussed could not be freed from one paradox, without either becoming inconsistent, or running into certain others; but when it is applied to the problem about the confirmation of '$(x)(\text{Raven } x \supset \text{Black } x)$', it can.

We do not claim more for it. We do not claim in particular that it is an acceptable solution to the second problem; on the contrary, if only because it depends on the Relevance Criterion of Confirmation, we think it is almost certain to entail paradoxical statements of confirmation which we have not noticed. The point of the present section was worth making, in the light of the earlier sections, only because the problem considered here has been so seldom distinguished from the problem about the ravens.

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Discussions

A REVISED DEFINITION OF NATURAL NECESSITY

I

I agree in the main with the opinions expressed in the note by G. C. Nerlich and W. A. Suchting (this *Journal*, 18, 1967, 233). I disagree with their presentation of some of these opinions as a criticism of doctrines which I am supposed to have published. They overlook, or at any rate they do not mention, the fact that some of the things they point out in their criticism were explicitly pointed out (and discussed) by myself in the very appendix (*The Logic of Scientific Discovery* = *L.Sc.D.*, appendix *x) which they criticise.

Though I am largely in agreement with Messrs Nerlich and Suchting, this does not mean that I do not find much to criticise in their note. But I shall confine myself to one point of criticism, and to a revised treatment of the problem of natural necessity.

Nerlich and Suchting's note is in its entirety directed against my italicised definition of natural necessity, printed in *L.Sc.D.* at the top of page 433, and quoted by Nerlich and Suchting at the end of their paragraph 1. Unfortunately this definition has no special name in *L.Sc.D.* and this has led to some (minor) muddles in my text. I will therefore call this definition in what follows 'definition (N°)'.

In their summary (paragraph 6) Nerlich and Suchting state three criticisms of my definition (N°):

(1) 'that it is at least verbally defective, . . . imposing no distinction on the class of universal propositions true of our world' (this point is argued in their second paragraph);

(2) that either a revised version would be 'equivalent to a view which Professor Popper wishes to reject' (that is, to what they call 'Kneale's position': see their fourth paragraph), or

(3) that a revised version would be a circular definition (see their fifth paragraph).

As far as (2) goes, I can only say that 'Kneale's position', as Nerlich and Suchting call it (I doubt whether this is indeed Kneale's position) was at any rate ruled out at some length in my text; and I disagree with Nerlich and Suchting that a revised version of (N°) would, if not circular, be equivalent to either 'Kneale's position' or else to what I believe to be Kneale's actual position.

On the other hand, Nerlich and Suchting indicate in their point (3) that if I reject 'Kneale's position' then my definition (N°) becomes circular. What they fail to indicate is that I not only reject Kneale's position but *that I have also pointed out the circularity myself*. For although they quote from *L.Sc.D.* my definition (N°) (Nerlich and Suchting, end of paragraph 1), they fail to quote from the same appendix the following critical comments of mine on the *definiens* of definition (N°) (*L.Sc.D.*, p. 435):

> Nevertheless, the phrase [from the *definiens* of (N°)] 'all worlds which differ (if at all) from our world only with respect to the initial conditions' undoubtedly contains the ideas of laws of nature. What we mean is 'all worlds which have the same structure—or the same natural laws—as our own world'. In so far as our *definiens* [the *definiens* of (N°)] contains implicitly the idea of laws of nature [(N°), and therefore the later definition (D) which depends on (N°)] . . ., may be said to be circular.

Having stated this I continue: 'But all definitions must be circular *in this sense* . . .'; and I go on to say: 'Our definition is not, however, circular in a more technical sense.'

I should like to explain here this technical sense more fully.

We may, in some formal system, choose the term 'P' as one of our undefined symbols; and we may, for example, decide to interpret the formula

$$a\epsilon P$$

to mean 'a is possible'. We may then continue (assuming negation is part of our system) to interpret '$\sim((\sim a)\epsilon P)$', or '$\sim a\not\epsilon P$', as 'not-a is not possible'. This allows us to *define logical necessity with the help of logical possibility* (as is customary)

(K) $$a\epsilon N \leftrightarrow \sim a\not\epsilon P$$

This definition of necessity or of 'a is necessary' is, like all definitions (except those which involve an infinite regress), circular *in a sense*, because 'possible' may be said to mean '*compatible with what is necessary*'. ('Possible' also means, clearly, 'not impossible', and 'impossible' means, just as clearly, 'necessarily wrong' or 'necessarily excluded', or something similar). Nevertheless, the definition (K) is *technically not circular*, because we have chosen P as our undefined term, and because we define N with its help.

Accordingly, anybody who adopts the position described by Nerlich and Suchting in their fifth (penultimate) paragraph, defining N (in the sense of naturally or physically necessary) with the help of the class of physically possible initial conditions (this class being used as an undefined term), would obtain a definition of N which like (K) would be circular *in a sense*, but which might be technically even less circular then (K).

Thus far my *criticism* of Nerlich and Suchting.

On the other hand, what I have quoted from point (1) of their summary seems to me to be a valid criticism of my definition (N°), which indeed is 'at least verbally defective'. In the rest of this note I shall attempt to improve it. I shall call my revised definition '(N)', and an interpretation or translation of it '(N′)'. From an intuitive point of view (N′) turns out to be very similar to (N°). Nevertheless there will be a considerable improvement; (N) or (N′) will turn out to be not only less circular than (N°) or (K) but a good deal less circular than I previously thought possible. For this reason I am very grateful to Nerlich and Suchting for their helpful criticism.

II

I shall make use of Tarski's theory of deductive systems. (See A. Tarski, *Logic*, *Semantics*, *Metamathematics*, chapters v, xii, and xvi).

If A is any set of statements, then $Cn(A)$ is the class of all logical consequences of A, or the smallest deductive system containing A as a subset: we have $A \subset Cn(A)$, and $Cn(Cn(A)) = Cn(A)$.

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(Tarski, op. cit. p. 343, calls the system $Cn(O)$ 'the set of all logically valid' statements, or 'L' while I shall use 'L' for the set of all statements of our object language.)

The sets of statements with which we operate can be interpreted in a 'realistic' way: we can speak (and I will do so later) of sets of possible, or of actual, facts, instead of sets of (logically) possible, or of true, statements.

I shall use the following metamathematical term T first introduced and defined by Alfred Tarski, and more recently defined by others. T is the set of all those statements (of some language L) which are true in our own world. It is easily seen that T is a deductive system: $T = Cn(T)$. On the realistic interpretation T becomes the set of all actual facts.

Note that T is a *consistent and complete* deductive system: there are statements or facts *outside* T (false statements or facts), which means that T is *consistent*; and if any statement not in T is added to T, T becomes inconsistent, which means that T is *complete*. I shall later call any such consistent and complete deductive system (in 'realistic' interpretation) a '*possible world*'. Thus T may be called 'our own world'.

The following notation will be used: AB will be the *intersection* of the two sets A and B; $A+B$ will be their *union*. Braces will indicate unit sets, so that $\{a\}$ will be the set containing the element a only. However, if a is a statement, the consequence class of a will, by way of abbreviation, be written '$Cn(a)$', instead of '$Cn(\{a\})$'. Also '$\rightarrow$' is sometimes used in our metalinguistic definitions as shorthand for 'only if', and '$\leftrightarrow$' for 'if and only if'.

I begin with some informal metalogical explanations. For simplicity's sake we interpret L as the class of all well-formed formulae (rather than of all expressions of L). Thus $a \epsilon L$ is taken to mean that a is either a well-formed statement or a well-formed statement function of L.

I now introduce my main auxiliary metalogical concept, the concept of *separateness*. This is closely related to *organicity* in the sense of Leśniewski and Wajsberg (and referred to by Tarski, op. cit. p. 45; see also Boleslaw Sobociński, *Yearbook of the Polish Society for Arts and Sciences*, London, 1956, p. 7).

We write '$\sigma(a)$' for a 'suffix' of the expression a, that is to say, for what remains of a after deletion of some or all of its quantifiers. We write '$a = b$' if a and b are the same expression, and '$\sigma(a) \not\subset b$' if and only if $\sigma(a)$ does not occur as a genuine part of the expression b.

Now we define:

(D1) A is a class of separated axioms of D (in symbols, $A \epsilon \alpha(D)$) if and only if

(1) $D = Cn(D) = Cn(A)$

(2) $(a)(b)(a \epsilon A \& b \epsilon D \rightarrow \sigma(b) \not\subset a)$

Note: The main point of this definition is to ensure that a 'separated' system of axioms A consists of axioms none of which is written as a conjunction (or a conditional) of two members of $D = Cn(A)$.[1]

We next introduce the idea of a separated *basis* (that is, a separated *independent* axiom system):

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[1] There are other ways (and perhaps preferable ones) to achieve this purpose: we may define a concept of 'separateness' adequate for our purpose using ideas other than organicity. (In fact I am not entirely happy about the utilisation, in my above definition of separateness, of what Sobocínski says on organicity.)

(D2) A is a separated basis of D (in symbols, $A \epsilon \beta(D)$), if and only if

$$A \epsilon \alpha(D) \& (a)(a \epsilon A \rightarrow (A - \{a\}) \notin \alpha(D)).$$

The purpose of these definitions—that is, of introducing the idea of a '*separated' basis*—is this. Suppose p is the statement, 'All planets move very nearly in elliptic orbits', and that it is in N; and that m is a contingent statement like our moa-example (*L.Sc.D.*, p. 427: 'All moas die before reaching the age of fifty years.') Then to be intuitively adequate, our definition must not put pm into N, since it is an obvious adequacy condition that N is a deductive system. To avoid this, we have to exclude such conjunctions from the set of 'separated' bases.

The following terms G, H, and K, will be used as undefined terms.

G is, intuitively speaking, the set of *all* general statements (of some language L) or general facts (expressible in L), that is, of all statements or facts which are general by virtue of their logical form: G is supposed to contain, more especially, all physical theories, and not only all universal statements which are naturally or logically necessary in our world, but also all contingently true universal statements (such as our moa-example, m); and further, also all false universal statements. We may assume that G contains universal implications (or conditionals), beginning with a universal operator 'for all x', where x may range over finite parts or regions of some world (or over all world points, or over all 'occurrences' in the sense of *L.Sc.D.*, section 23, or a 'realistic' interpretation of this section). The main thing is that in no statement or fact belonging to G may occur an individual constant or proper name (or an individual class name: cf. *L.Sc.D.*, section 14; or a predicate such as Quine's 'Socratizes'), or anything that may specify an individual part or region of some world.

This explanation of G is not very satisfactory; a more precise explanation would be still more closely language-dependent, but it could be carried out by introducing some sufficiently rich artificial object language.

H is the set of all logically possible singular statements, including initial conditions I (see below). It will be interpreted broadly: I will assume that $H \subset L$ and that H may be infinite; also that it may include what are usually called 'boundary conditions', that is to say, propositions or statements asserting the existence or non-existence of certain conditions in certain individual (*L.Sc.D.*, section 14) or singular parts or regions of the world. No member of the set H may assert, however, that such conditions exist in *all* parts of the world or in *no* parts of the world, since statements of this kind will be regarded as general or universal statements. But H may, for example, contain statements asserting that certain conditions—such as the existence of animals of a certain kind—are realised in certain finite parts of the world (e.g. in a certain spatio-temporal region), and *in no other part*. The class of statements of this kind I denote by 'K', in contradistinction to initial conditions in the narrower sense, to be defined below, and denoted by 'I'.

Note that although G and H are assumed to be exclusive ($GH = 0$) they are not assumed to be exhaustive ($G+H \neq L$). Neither of them is assumed to be consistent, and they are not deductive systems.

I shall make use of no undefined terms other than G, H, and K.

We now introduce a preliminary definition (Prel): we define the deductive systems U and V. The deductive system U is the system of all facts entailed by the set of all *true* general statements *not belonging* to H; and the deductive system V is the system of facts entailed by all *true* initial conditions.

(Prel 1) $$U = Cn((TG)-H).$$

Note that U contains elements which are not universal; for example, instantiations resulting from the substitution of individual names.

We now proceed to define the set I of initial conditions (in the narrower sense). To this end we first consider the set J, defined as the largest set of *conditionals* which belong to UH. Thus J may also be described as the largest set of (true) singular conditionals obtainable within U by instantiation. (Note that $a \supset b \epsilon J$ if and only if $\sim b \supset \sim a \epsilon J$.) The set of the *antecedents of J* belonging to H is the set I of initial conditions (in the narrower sense: $I+K$ may be described as the set of initial conditions in the wider sense).

We are now ready to define the deductive system V as the system derivable from the *true* initial conditions in the wider sense:

(Prel 2) $$V = Cn(T(I+K)).$$

Note also that although U and V are semantical terms, *they are not modal terms*.

With the help of V and $\beta(U)$, that is the class of *all 'separated' bases* of U, we can now define M ('M' for 'matrix'); that is to say, a 'separated' (but not in general an independent) axiom system M for the system N ($N = Cn(M)$) of all *naturally or physically necessary facts or statements* (or statement functions).

(M) $$a \epsilon M \leftrightarrow (\mathrm{E}A) A \epsilon \beta(U) \;\&\; a \epsilon A \;\&\; a \notin Cn((A-\{a\})+V)).$$

Now we define the deductive system N of naturally necessary facts:

(N) $$N = Cn(M)$$

These definitions (Prel), (M), and (N) are, obviously, *technically* non-circular: none of the defining terms is in any sense a modal term. It is of some interest, however, to ask ourselves *in which sense* the definition of N is still circular. The answer is that we must assume our language L to be *adequate for the particular purpose of defining N*. That is to say, L must be sufficiently rich and articulate to allow a division of the true general and 'separated' statements into two exclusive classes, one consisting of the naturally or physically necessary ones, and one of the naturally or physically contingent ones. Not every language will satisfy this condition. To take a trivial example, we assume that a language contains a primitive predicate P which is to be interpreted as 'either planets or moas', and two predicates, Q and R, to be interpreted respectively as 'always moving in near-elliptic orbits around a central body' and 'dying before fifty'. Then we have to express our two standard examples p and m by '$(x)((Px \,.\, Qx) \rightarrow Qx)$' and '$(x)((Px \,.\, Rx) \rightarrow Rx)$' respectively; and these will both be tautological, and therefore (logically) necessary.

Thus we cannot assert that our definition will be adequate for every language L in which we can express statements which are intuitively (or in intention) naturally necessary, or else contingent. We can only say that it will be adequate for every language which is adequate *for the particular purpose* of distinguishing between naturally or physically necessary statements and contingent statements. This means that our definition is circular in a sense; yet I regard this as a quite unexpectedly low degree of circularity.

Another difficulty is of a very different character. A strong assumption on which the success of our construction depends is that U is finitely or recursively axiomatisable. (If it is, it has obviously a basis and, in every appropriate language, a 'separated' basis also.) Now, this question does not so much depend on L, but rather on T, that is to say, on our world. Since not even arithmetic is finitely axiomatisable, it appears that U will not be either. However, there are ways to get round this difficulty: for example, we might 'subtract', as it were, the mathematics from U by including it among the logic (thus packing it, as it were, into the symbol '*Cn*' for logical consequence, or into $Cn(\mathrm{O})$; concerning the relativity of logical consequence, see Tarski, op. cit. chapter 16, pp. 418-20).

There is another method of dealing with this difficulty, which also utilises an idea of Tarski's (cf. Tarski, op. cit. p. 362, theorem 25). Tarski's idea allows us to represent any non-axiomatisable system D by the sum of some infinite series of strictly increasing axiomatisable systems X_i (and thus also of systems $X_i \epsilon \beta(Cn(X_i))$, that is to say, of bases). This suggests the following definition:

(D3) A is a sequence of strictly increasing 'separated' bases of subsystems converging to D (in symbols, $A \epsilon \gamma(D)$) if and only if, for every natural number n, $(\mathrm{E}B_n)(B_n \subset B_{n+1} \;\&\; B_n \neq B_{n+1} \;\&\; B_n \epsilon \beta(Cn(B_n)) \;\&\; A = \sum_n B_n \;\&\; D = Cn(A))$.

Now we can replace (M) by (M′), which is exactly like (M) except that 'β' is replaced by 'γ'.

III

It is clear from the start that our construction has no practical significance: my aim has been to see how far we can 'rehabilitate' the concept of natural necessity, somewhat in the sense in which Tarski may be said to have rehabilitated the concept of truth. I am interested (as I am in the case of the concept of truth) not so much in finding a concept which is effectively applicable (except for very special languages, there is no criterion of truth), but rather in showing that we can use the concept intuitively without talking nonsense.

The deficiency of my old definition (N°) can best be seen by comparing it with the following definition (N′) which incidentally also shows how my old definition can be repaired (and thus that it *can* be repaired). (N′) is a close and, as it were, a largely 'realistic' translation of (N).

(N′) a is a naturally or physically necessary fact (or statement, or statement function) of our own world T if and only if a is satisfied in every *logically* possible world which satisfies a set M of 'separated' and universal facts, consisting of every fact b such that to b corresponds an axiom b' with the following properties: (1) b' belongs to one or another independent set A of 'separated' universal axioms *which are all satisfied in every possible world differing from our own world (if at all) only with respect to initial conditions (in the wider sense)*; and (2) b' *is not* satisfied in every possible world W in which all the other axioms of A are satisfied, even if it is stipulated that *all those initial conditions* hold in W which hold *in our own world.*

I wish to express again my gratitude to Messrs Nerlich and Suchting for their criticism and to Dr Mary Hesse for spotting a mistake in an earlier version of this note. As always, David Miller, my former research assistant, has been of the greatest help in criticising and checking my efforts. But my greatest debt is to William Kneale, who (in this *Journal* 12 (1961), 99–102) corrected some of my formulations most convincingly and understandingly.

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A NOTE ON RELATIVISTIC PHENOMENA IN AN ETHER THEORY

Professor Popper's comment[1] on the relativistic contraction phenomenon is based on an incomplete appreciation of the implications of an ether (or substratum) assumption. It is true that the substratum concept provides a basis for absolute motion and hence for absolute effects associated with such motion. But these may be considered as secondary effects stemming from a more fundamental consequence, viz. that a substratum provides a basis for the propagation of light (and of other forms of energy), such that its velocity is independent of the velocity of its source and has the same value in every direction relative to a basic reference frame associated with the substratum. In other words, the propagation of electromagnetic and other forms of energy is isotropic with respect to this basic reference frame.

This was the fundamental property of the ether as conceived by Lorentz, Fitzgerald and their predecessors. However, and this is rather surprising, none of these proponents of an ether appeared to grasp the full implication of this fundamental property and of its immediate consequence, viz. that the propagation of energy must therefore be anisotropic in respect to any reference system moving relative to the substratum or basic reference frame. By applying Maxwell's theory to the problem of an electron moving relative to the ether, Lorentz did in fact invoke (but only implicitly) the consequences of the anisotropy effect on such an electron; however he never drew direct attention to the role of the effect, and with the triumph, shortly afterwards, of Einstein's alternative approach, the implications of an ether remained ignored and neglected.

And so the matter stood for over fifty years. It remained for the late Dr G. Builder[2] to reveal the significance of the anisotropy effect and to suggest that, in terms of Einstein's measurement conventions, this effect together with the Fitzgerald contraction provide a complete and satisfactory interpretation of Special Relativity. The validity of Builder's approach and claims were later demonstrated by the author.[3] It was shown that the interplay of the anisotropy effect and the Fitzgerald contraction resulted in a credible time-dilatation effect; and that as a result of the interaction of these effects, observers employing Einstein's measurement conventions would appear to find that their *measure* of the velocity of light was the same in respect to all inertial systems, and that the Lorentz transformation related the observations of an event by observers associated with different inertial systems. In terms of these effects the reciprocity phenomenon is rendered physically intelligible and so is the differential time-effect for a traveller making an out-and-return journey.

Following the work of Bastin,[4] it was further proposed that the Fitzgerald contraction is also essentially due to the anisotropy effect since (and this is consistent with Lorentz's different approach leading to the same result) this latter effect must modify, according to a stationary observer, the inverse-square laws associated with energy propagation.

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[1] K. R. Popper, 'A Note on the Difference between the Lorentz-Fitzgerald Contraction and the Einstein Contraction, this *Journal*, 1966, **16**, 332-333.

[2] G. Builder, 'The Constancy of the Velocity of Light', *Aust. J. Phys.*, **11** (1958), 457-480.

[3] S. J. Prokhovnik, 'The Case for an Aether', this *Journal*, **14** (1963), 195-207. A more formal exposition of the mathematics of this approach appeared in *J. Aust. Math. Soc.*, **5** (1965), 273-284 and **6** (1966), 101-105.

[4] J. A. Bastin, 'An Extension of the Newtonian Law of Gravitation', *Proc. Camb. Phil. Soc.*, **56** (1960), 401-409.

It is remarkable (though not necessarily fortuitous) that these various effects should be exactly compensatory such that both the uniform motion in the ether and the resulting complex of effects remain entirely concealed from the moving observer. Since the various effects are closely inter-related and have a common origin, this result is more logical and easier to comprehend than the alternative of having no ether and hence no unique basis for energy propagation, together with the contradictions this leads to. The association of compensating phenomena with uniform motion may be considered as an extension and complement of Newton's action-reaction law; and the exact equality of the opposing effects generated in these circumstances is certainly no more remarkable than the exact equality of magnitude of the fundamental electric charges and their resulting complete concealment in the normal state of matter.

Finally, it is interesting that present cosmological evidence (much more extensive than available to Einstein) provides excellent grounds for the assumption of a universal substratum,[1] and that this is also Keswani's conclusion[2] on quite different grounds. Keswani's critical historical analysis is particularly valuable in showing that Relativity theory is not the final and completed edifice of one man, but rather that it resulted from the interplay of many minds, ideas and observations. His approach treats Relativity as a theory which (like other physical theories) may never take a final form, but will become richer and more valuable as our understanding of it develops in the light of new observations and discoveries.

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REPLY TO MR KUMAR

Since Mr Kumar's article ('Logic and Inexact Predicates', this *Journal*, **18** (1967), 211) is almost wholly devoted to criticisms of my approach to the analysis of empirical inexactness (in the first part of *Experience and Theory*, London 1967), it may seem appropriate that I should indicate my answers to his objections. This can be done rather briefly: (i) Kumar (p. 211) disputes my 'claim to have given the logic of empirical discourse'. But I emphasised that the three-valued logic which I employ and whose values I interpret as 'true', 'false' and 'neutral', is itself an idealisation by means of which I try to draw attention to features of empirical discourse usually left out of sight (e.g. op. cit. p. xi, p. 232). (ii) Kumar (p. 213) objects that I do not produce a single truth-functional tautology of this logic. But, as is clear from the truth-tables, it contains no forms all substitution instances of which are true—only forms of biconditionals all substitution instances of which have the same truth-value on both sides of the connective of biconditionality. This is one of the reasons why I defined and exemplified a weak notion of logical truth which is applicable to some propositions whose truth-value does not depend on their neutral components (op. cit. pp. 41 ff.). (iii) I see no force in Kumar's objection (p. 217) to my distinction between the provisional and final evaluation of propositions with neutral components, especially as he himself grants a place to 'provisional neutrality' in a logical analysis of empirical discourse. (iv) Kumar (p. 220) objects to my distinguishing between neutral and undecidable candidates for membership of a class. But the distinction corresponds to a difference. A question about the admission or refusal of membership in accordance with a set of rules is undecidable if, and only if, neither the admission nor the refusal accords with the rules It is neutral if, and only if, both admission and refusal accord with the rules (op. cit. pp. 27, 93). (v) Kumar (p. 221) objects to my calling a set of qualifications and disqualifications for membership 'complete' if in accordance with it some candidates are neutral. But such a set may be complete in the sense that every candidate for membership is positive, negative or neutral (and none undecidable). This does not mean that it may not be 'supplemented' by a rule for dealing with the neutral candidates. (vi) Kumar (p. 221) rather labours the obvious fact that the refinement of experimental techniques normally results in a reduction of the neutral cases of those concepts which are operationally defined by reference to these techniques. But I have, of course, not denied this (see e.g. op. cit. p. 57). My point was simply that whenever inexact concepts are accommodated in a theory (embedded in classical logic) they are *ipso facto* modified by the removal of their inexactness. (vii) Kumar's own view that 'neutrality like contingency is carried from premisses to conclusion' (p. 222) is patently mistaken both for the classical and the three-valued logic. (*a*) In accordance with both systems the conjunction of a true contingent and a true non-contingent proposition is contingent and yet materially implies the non-contingent conjunct. In the classical logic the corresponding logical implication also holds. (*b*) In the three-valued logic any neutral proposition implies any true proposition. In the classical logic all propositions—whether or not their truth-value is known—are assumed to be true or false. The question of the transference of neutrality (in my sense) does not even arise.

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[1] S. J. Prokhovnik, 'A Cosmological Model of Light Propagation', *Proc. Camb. Phil. Soc.*, **60** (1964), 265-271.

[2] G. H. Keswani, 'Origin and Concept of Relativity (*III*)', this *Journal*, **16** (1966), 273-294.

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Reviews

Paul K. Feyerabend and Grover Maxwell (eds.): *Mind, Matter, and Method: Essays in Philosophy and Science in Honor of Herbert Feigl* (Minneapolis, University of Minnesota Press, 1966. 78*s*.). Pp. v+524.

Part 1: *Philosophy of Mind and Related Issues.*

My main aim in writing an essay on this Part of the festschrift is to infect others with a sense of malaise. For some months I have been reading nineteenth-century debates on the mind-body problem, and I welcomed this opportunity to contrast the earlier literature with a series of commentaries on Feigl's essay, 'The "Mental" and the "Physical" ' (*Minnesota Studies in the Philosophy of Science*, II, Minneapolis, 1958, p. 370), which the publishers of a paperbound reprint (with postscript) remind us that a reviewer in this *Journal* called 'a super-collossal survey of the mind-body problem'. Early in that essay Feigl expressed a sense of weariness which is not unlike my own affliction: 'But despite the considerably greater scientific and logical sophistication in recent treatments of the issue, it is somewhat depressing to note that the main philosophical positions still are these: materialism, mentalism, mind-body interactionism, evolutionary emergence theories, psychoneurophysiological parallelism (epiphenomenonalism, isomorphism, double aspect theories), and neutral monism' (*ibid.*, pp. 370-1). Feigl reports that this was his fourth attempt to arrive at 'an all around satisfactory clarification' and that he had concerned himself 'seriously and repeatedly with the problem for about thirty-six years' (*ibid.*, p. 387). Whatever the fate of the essay may be, its bibliography is the most comprehensive to appear since 1901 (*Dictionary of Philosophy and Psychology*, ed. James M. Baldwin, New York, vol. iii, pt. ii, pp. 1091-9).

Feigl argues that 'The "meat" of an adequate solution of the mind-body problem will consist in a specific analysis of the characteristics and the relations between the attributes of the mental (especially the phenomenal) and the physical (specifically the neurophysiological).' He goes on to say that 'The most controversial, tangled and perplexing questions concern, of course, the distinctions made rightly or wrongly in the Cartesian and in the subsequent dualistic tradition between the mental and the physical . . .' (op. cit., p. 396). But neither Feigl nor (with one exception) those who have written to honour him seem to have considered seriously the possibility of examining and rejecting the basic Cartesian, dualistic context within which the problem has traditionally been cast. Instead, Feigl discusses a number of distinctions which have been made between the mental and the physical in some detail (op. cit., pp. 396-419). He says, 'It is truly a cluster of intricate puzzles—some scientific, some epistemological, some syntactical, some semantical, some pragmatic. Closely related to these are equally sensitive and controversial issues regarding teleology, purpose, intentionality, and free will' (op. cit., p. 373). Indeed, there are so many puzzles packed into one problem that I am beginning to feel that debates on the mind-body relationship are becoming suspiciously like the early nineteenth-century question, 'How did all those animals get into one ark?'

In what follows I propose to mention the ten articles which make up the first part of the Feigl festschrift and then to single out two of them as a basis for a suggestion that the context within which mind-body issues are considered might fruitfully be changed.

About half of these essays have the air of occasional pieces commemorating controversies which, though not resolved, have lost their former place at the centre of philosophic debate: psychoanalysis, anti-behaviorism, and operationism. Rudolf Ekstein calls for greater communication between psychoanalysts and philosophers of science. Wolfgang Köhler (whose death has occurred since this volume appeared) offers a behaviourist-induced defence of the 'directly accessible' perceptual world. C. C. Pratt defends free will on the basis of his consciousness of it. Wilfred Sellars reprints a chapter of a work published three years earlier in which he attacks phenomenalism and offers a prolegomena to a scientific realism. S. S. Stephens provides some interesting reminiscences on the early development of operationism and then proceeds to give examples of the more tolerant attitudes of a movement now in middle age. He concludes that the operational attitude is typical among working psychophysicists and now 'resembles behaviorism minus the early phobias. Psychophysics involves the study of behaviour, but there need be no forbidden terms' (p. 232).

The remainder of the contributions are directly concerned with Feigl's long essay, in which he argues 'that the central puzzle of the mind-body problem is the logical nature of the correlation laws connecting raw feel qualities with neurophysiological processes' (op. cit. p. 416, cf. p. 372). His own position is well-known: an identity theory in which considerable care is taken to specify the sense in which the term 'identity' is being used. In reply, Michael Scriven offers a brief but provocative statement of some limitations of the identity theory. Bruce Aune, on the other hand, defends the identity thesis, while arguing that phenomenal qualities are not really physical qualities (p. 38). While his line of reasoning is interesting, I shall pass it by for two reasons: May Brodbeck's is much more elegant, and Aune's criticism of Feigl on private languages is much more telling. I, like Aune, do not have a private, phenomenal language, and P. F. Strawson has convinced me that I cannot have one. I also share Aune's suspicion that Feigl has not got one either and that his argument about 'raw feels' can and should be conducted without reference to private languages (p. 21). Feigl made it clear in 1959 that he could glean very little from Strawson's essay and book on this topic. ('Mind-Body, *not* a Pseudoproblem', *Dimensions of Mind*, ed. S. Hook, New York, 1960, p. 26; P. F. Strawson, *Individuals*, London, 1959, and *Minnesota Studies* II, p. 330. On private languages, see also P. F. Strawson, Review of Wittgenstein's 'Philosophical Investigations', *Mind*, 1954.) Just as Aune finds Feigl's views on private languages incoherent, Meehl addresses the same basic issue (with similar incredulity) with respect to whether or not phenomenal qualities are intersubjective. I believe that Strawson has shown that the ascription of any subjective state to oneself implies criteria for its ascription to others, and conversely; these criteria must be public.

Wallace I. Matson, without considering the detailed analysis of what Feigl means by identity, argues rather cryptically for a redefinition of the issues. He uses Aristotle as a text for his own position: that the notion of sensation should be drastically modified so that we are not forced to dualist or reductionist solutions to a problem which has the air of a hoary old exercise for undergraduate classes. While I share Matson's interest in alternative approaches to the problem, I do not follow his reasoning.

The real centre of Feigl's view is the sense in which he uses the term 'identity', and this topic provides the themes of the two most considerable essays of the ten, those of Paul E. Meehl and May Brodbeck. They are both excellent treatments, and they complement each other nicely. The two most interesting aspects of Feigl's theory are (1) the claim that the identity thesis, though philosophical, is a piece of *empirical* metaphysics and thereby open to empirical refutation and (2) the particular conception of identity which his theory requires. Meehl devotes seventy-five pages to an exhaustive exploration of the findings of thought experiments employing an autocerebroscope, i.e., a machine which provides a complete physiological specification of one's brain states according to the laws of the completed science of Utopian neurophysiology. I am not certain that Meehl's analysis is genuinely aided by his mentioning particular findings in neurophysiological and psychological science at such length and in such detail, nor do I see why the reader should be subjected to neologisms such as 'malcommunication' and 'discorroborate', but I do agree with his conclusion: 'The net result of these ruminations is, I think, to leave the identity theory in pretty good shape as an internally consistent hypothesis of "empirical metaphysics" ' (p. 164).

Meehl's essay ends with a brief discussion of what he considers an important objection to identity which, as he says, is difficult to state clearly, and he is quite diffident about his attempt to discuss it (pp. 164, 178). Crudely (the objection runs), one feels that a theory which says that a raw feel 'red' *is* (*sic*!) a brain state somehow cheats. The explanation eliminates the qualitative aspect; what is to be explained has disappeared. The argument is complicated and difficult to follow, but the objection is worrying. Indeed, Feigl does not seem at all clear about it. There are apparent uncertainties about what is being identified with what at a number of points in his argument (e.g. op. cit. pp. 438, 440, 442-3, 442 n., 445). Whether it is Feigl or Meehl (e.g. pp. 112-13, 153, 158, 165) or Young who is muddled is, fortunately, unimportant, since any or all of us can turn to the brilliant and lucid exposition of the meaning of the identity theory by May Brodbeck.

Meehl is quite right to point out that the identity theory fails to mention the perceptual qualities of things in its explanations, but he ought not to worry about it. The whole point is that the identity theory makes psychology conform to the paradigm of scientific explanation which was (not all knowingly) laid down in the seventeenth century by Galileo, Descartes, Newton and Locke: from the phenomena of matter and motion to explain the other phenomena. In physics, yellow *is* (*sic*!) light waves of a certain length. The physicist does not mention the nervous system and the perception of a secondary quality as a part of his explanation. The psychologist wants to know about just that bit of nature, and if he is to conform to the rules of scientific explanation, he must apply an identity theory. It has taken a very long time to get to the point that we can strip the mind-body problem of all sorts of epistemological and ethical issues (which were themselves crucial in the formulation of just *this* paradigm of scientific explanation) and to see that in science to explain *means* to specify the physical state which obtains when a given experience is occurring. It is extremely difficult not to lose sight of this point, and the most impressive thing about Brodbeck's essay is the surefooted and authoritative way in which she leads the reader through a number of traditional puzzles. (The apparent nonsense of p. 48 is common sense by p. 52, albeit after several readings.)

In a scientific explanation, 'Talk about physical states replaces talk about physical objects' (p. 44). The sense in which the mental and the physical are identical is a weaker sense of the term than one suspects Feigl had in mind. Brodbeck calls this sense 'model identical', as distinguished from sameness and from logical identity. By a logical extension of the latter notion, 'we can further explicate the use of "identity" in contexts where two notational systems or languages are so related that one is a model for the other. Something is red if and only if something else is a certain light wave. A fact expressed by a sentence in one system is "identical" with that expressed by a sentence in the model, if the corresponding sentences are extensionally equivalent. This is what I have referred to as "model identity". . . . the replacement, for purposes that vary with the relevant languages, of one way of speaking about certain things with another way of speaking about them . . . the language of physical objects replaces that of perceptual objects for the purpose of scientific explanation of the visible by means of the invisible' (p. 53). She goes on:

> *The scientific world-picture bypasses the perceptual world.* The states of the particles cause our conscious states. But this is not literally true. We are not only left with particles and minds. Physical states have only physical effects. For, of course, there are no minds either. Or, rather, in keeping with our enterprise, 'There really are no minds'. With increasing knowledge of the behavioral and neurophysiological perceptual correlates of consciousness, these correlates will be model-identified with, reconstructed as, or replaced by—the three phrases mean the same—physical states (pp. 54-5).

One of the apparent consequences of the seventeenth-century paradigm of scientific explanation—and the cause of many a mind-body puzzle—was the belief that 'Colors and other perceptual characters of things are mental or in the mind' (p. 41). Brodbeck is at pains to refute this while explaining the meaning of identity. She recalls the genesis of the distinction between primary and secondary qualities and the role of measurability in the development of seventeenth-century ontology and epistemology (p. 48).

> Measurable versus nonmeasurable was the crest of the slippery slope from primary versus secondary, over objective versus subjective, to reality versus appearance, that ended with ordinary perceptual objects being stripped first of their colors and finally of all perceptual characters. By the assimilation of nonqualitative perceptual characters to sensations, to such things as our feelings of pain or of warmth, the whole perceptual object was ultimately to be located in the mind (p. 49).

I have quoted this at length in the hope that this (accurate) depiction of the developments which led to the traditional frame of reference of the mind-body problem will strike others as strangely as it does me. Brodbeck's explanation of the meaning of identity within this dualistic context is an impressive extention of the paradigm to the bitter end. Must we forever see the world in this way, however? Feigl closes his essay with an invitation to look elsewhere: 'Despite my valiant efforts to deal with what strike me as important and baffling questions, I may of course not even have touched on other facets which some of my readers might consider as the essential problems of mind and body. May others come and deal with them!' (*Minnesota Studies*, II, p. 483).

I have a colleague who was once glancing through the umpteenth book dealing with the conceptual problems of modern physics. He cast it aside in despair and said, 'I'm so tired of the same old controversies. I want to know what matter is—or at least what it's like. This book isn't getting me closer.' The essays by Feigl and his commentators give me an analogous feeling. I want to know what mental functions are or at least what they are like. Put more honestly, I want to know how men became as they are. Science has shown itself the most fruitful means of finding out about many aspects of nature. However, great difficulties have been encountered in applying the methods and assumptions of science to the study of certain aspects of man and especially those functions called 'mental'. The sets of abstractions which have worked so well in physics, astronomy, chemistry and certain branches of biology have caused great difficulties when applied to man. The identity theory shows what it means to apply them strictly to man, and perhaps that's that. However, *the* problem, as it seems to me, is the place of so-called mental functions in nature. How do we make sense of mental functions? The answer to this question would seem to depend only in part on the findings of neurophysiology. It should also include a close analysis of the basic concepts of evolutionary biology.

Feigl stresses the relevance of scientific data to the mind-body problem and complains that 'ordinary language' philosophers make scarcely any reference to 'the facts and regularities of neurophysiology' (*Dimensions of Mind*, p. 26). As I suggested above, it is not clear that straightforward knowledge of the findings of neurophysiologists will help the mind-body philosopher. Philosophically interesting articles do turn up: Meehl mentions one which suggests that normal subjects can be taught to *experience* the wrong colour. 'Raw feels' may not be sacrosanct after all (p. 133). Even so, there is a surprising faith in neutral, positive science lurking behind the injunction to bring the results of science to bear on this philosophical problem. In the nineteenth century eminent neuroanatomists and neurophysiologists denied that they could discover any physical continuity between the cerebral cortex ('the organ of mind') and lower centres (automatic, reflex). Similarly, we have not yet got free of the perennial questions regarding the nature of animal learning. Rational? Insightful? Merely reflex? Descartes had a convenient solution which few would now accept, and those who do would also apply it to man. The criterial of rationality are routinely denied to animals—ability to make general statements and statements about the past. The argument that genuine linguistic behaviour is restricted to man has been a cornerstone of dualistic thinking. Is it now a *finding* of science or the product of an assumed dualism? (See, for example, *Minnesota Studies*, II, p. 411). Cartesian dualism has often operated as a constraint on what scientists find themselves able to think, what experiments they perform and which aspects of the 'data' they attend to. In philosophy the concept of intention has received considerable attention of late and is now (along with the implications of cybernetics) at the centre of interest in the philosophy of mind. These debates are the heirs of the problems of free will. Descartes casts a very long shadow indeed.

The issues which *do not* seem to find an important place in debates on mind and body are those involved in biology and particularly the evolution of mental functions. Since 1855 and certainly since the advent of cerebral cortical stimulation in the 1870s it has been clear that mental functions evolved and that experience and behaviour can be caused by certain states of matter. Yet the concepts of biology (of which, *N.B.*, psychology and neurophysiology are both branches) have received scant attention from philosophers: adaptation, function, biological property, utility, organism. In the writings of Feigl and his commentators, evolution seems to be the root for an adjective which modifies 'emergentism'. It is mentioned in other ways (e.g., p. 213; *Dimensions of Mind*, p. 29), but seldom. Similarly, the collaboration between psychoanalysts and philosophers of science might attend to the ways in which Freud's belief in psychophysical parallelism came into conflict with his avowed aim of creating a biological science of human adaptation and maladaptation.

As I said at the outset, recent collections of essays on the mind-body problem have given this reviewer a sense of acute malaise. Feigl has given us a very detailed accounting of the traditional issues, Meehl has audited it, and Brodbeck has made as much sense of it as now seems possible. But I am still left wondering how all those animals got into one ark and am beginning to think about investing in other vessels.

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Part II: *Induction, Confirmation, and Philosophical Method*

In considering this Part, I shall concentrate mainly on the papers relating to content and confirmation, empirical meaning, and conventionalism.

In 'Probability and Content Measure', Carnap gives a characteristically careful analysis of Popper's thesis 'the larger the probability of a statement, the smaller its content measure' (p. 250). Taking content measure, as Popper does in some of his work, to be the reciprocal of *initial* probability, it is easy to show that the thesis does not hold in general for *present* (posterior) probability. Neither is it the case, as Popper seems to think, that the two rules

(*a*) Choose the hypothesis with the higher content measure,

(*b*) Choose the hypothesis with the higher probability,

are incompatible in all cases. In fact, if the probabilities are equal, (*a*) is acceptable, and if the content measures are equal, (*b*) is acceptable, and in agreement with good scientific practice. We are not, however, always interested in applying (*a*) and (*b*) under these restricted conditions, so Carnap goes on to define *optimum interval hypotheses* for a normal distribution, which are such that no other hypothesis with the same probability has higher content, and no other hypothesis with the same content has higher probability. For these optimum hypotheses, (*a*) and (*b*) *are* incompatible, and Carnap concludes that neither should be adopted, but that a compromise should be devised which takes account also of utilities. However, with regard to theoretical rather than practical contexts, Carnap suggests that the question is not well posed in terms of 'choice' or 'acceptance', since information is lost in giving an answer to this question. Scientists also wish to give logical probabilities to hypotheses, and these formulate evidential knowledge more completely than acceptances and rejections.

Carnap's paper is an elegant exposition of the view that there are *three* dimensions to the problem of applying a confirmation theory. These are *posterior probability*, *content measure*, and *utility*. All require extensive pre-formal analysis. Roughly, probability corresponds to what is most likely to be *true*, content to the power a hypothesis would have if true or partly true, and utility to what we are prepared to risk to get both truth and power. To recognise these three independent dimensions is to perceive that many of the disputes about measures of confirmation are unnecessary, because based on an over-simple analysis of the various factors involved. In particular, recognition of the three dimensions has no tendency to show that development of a theory of logical probability is worthless. Indeed, even in practical contexts, where content and utility as well as probability are taken account of, any set of admissible rules for the choice of hypotheses can be represented in terms of logical probability alone by choice of a suitable initial distribution, by the general equivalence of Bayesian and decision methods. But of course, Popper's dispute with Carnap lies deeper than this, because for Popper expectation of truth as measured by probability is not an acceptable concept. This particular dispute will not be settled within the framework Carnap develops in this paper.

Happily, Popper's paper 'A Theorem on Truth-Content' bears precisely on this point. Popper considers the set of statements A which are consequences of a hypothesis a. Whether true or false, a has among its consequences some true statements. Let their set A_T be the *truth-content* of a, or $A_T =_{\mathrm{df}} A \times T$, where T is the set of true statements. Suppose T is non-axiomatisable (for example, it contains all number theoretical true statements), a is a false hypothesis, and b one of its consequences. B is the set of consequences of b, B_T its truth-content. A and B are assumed finitely axiomatisable. We can now ask the following questions:

(i) Can we have $b \epsilon A$, $A \neq B$ and $A_T = B_T$? If we can, then b will be a preferable hypothesis to a, since its truth-content is the same as a's and its falsity-content less.

(ii) Can there be a hypothesis c which contains a (is logically stronger than a), but has the same truth-content as a? If so, then a will be preferable to c.

If the answers to both questions are affirmative, we shall be in the position of preferring the logically weaker hypothesis, which is contrary to Popper's general directive to prefer stronger hypotheses in cases of choice.

The answer to both questions is negative. Popper shows how Tarski's calculus of deductive systems yields the following theorem: $A_T = B_T$ iff $A = B$, and $A_T > B_T$ iff $A > B$. The same holds also for any complete non-axiomatisable system X in place of T (for example, X may be a 'possible world'). Popper concludes *that for hypotheses related as a*, b *and* c, there is a one-to-one correspondence between contents and truth-contents, and he therefore takes comparison of contents to be a '*rough first comparison of the verisimilitude*' of hypotheses (p. 313, his italics). This, he claims, explains why 'only hypotheses whose content exceeds that of their predecessors have much chance of being considered as promising lines of progress in science—as hypotheses that may lead to an increase in verisimilitude'.

To see the significance of this theorem it is necessary to recall that in Popper's system we cannot compare the actual truth-contents of as-yet-unfalsified hypotheses, because no measure of truth or probability can be drawn inductively from the evidence. (This is why Carnap's posterior probability is not a relevant measure for Popper.) Thus, if 'verisimilitude' is to be a measure of truth-content, there is no way of directly assigning verisimilitude to competing hypotheses. But the theorem tells us that truth-content is proportional to content, and a measure of the latter can be assigned—in his most recent writings Popper favours the content-measure $ct(a) = 1 - \mathrm{p}(a)$, where $\mathrm{p}(a)$ is the initial probability of a. Since we clearly want hypotheses to have high truth-content, the theorem seems to justify choice of a hypotheses, say a, having higher content in preference to another, b, which is a member of the consequence class of a and hence has lower content and lower truth-content.

The trouble with this reasoning is that it is not clear how it applies to realistic choice situations in science. There is certainly something paradoxical about Popper's stated conclusion from the theorem, namely 'Of two as-yet-unfalsified hypotheses, choose the logically stronger because it has higher truth-content'. We want to object 'But perhaps it has higher *falsity*-content also?' Indeed Popper's original rationale for choice of the stronger hypothesis was that it had greater possibilities of falsification if false. What exactly is going on here?

The objector's question does have force. For Popper's own definitions of measures of 'content', 'truth-content', and 'falsity-content' in *Conjectures and Refutations*, pp. 392-4, show that nothing can be concluded about the relative falsity-content, measured by ct_F, of competing hypotheses from their relative content. From the theorem and these definitions we have

$$A_T > B_T \text{ iff } A > B$$

and

$$\mathrm{p}(a_T) < \mathrm{p}(b_T) \text{ iff } \mathrm{p}(a) < \mathrm{p}(b)$$

But

$$ct_F(a) = 1 - \frac{\mathrm{p}(a)}{\mathrm{p}(a_T)} \text{ and } ct_F(b) = 1 - \frac{\mathrm{p}(b)}{\mathrm{p}(b_T)}$$

from which nothing follows about the relative values of $ct_F(a)$ and $ct_F(b)$. Moreover, if we take as an elementary example, a is the statement 'All heavenly bodies move in ellipses', and b is the statement 'All planets move in ellipses', we have $B = B_T$ and $A > B$. But it is certainly *logically* possible that no bodies except planets move in ellipses, in which case we should have $A_T = B_T$ and $ct_F(a) > ct_F(b) = 0$. That this example does not require the non-axiomatisable system considered in the theorem is beside the point. The example could easily be modified to include some number-theoretical statements, and though the theorem demands that we could then not have $A_T = B_T$ (because $A > B$), it would still be the case that we should intuitively require $ct_F(a) > ct_F(b)$ in the important respect that there are far more actual falsifiers of *a* than of *b*. And nothing that has been said is sufficient to show that the 'extra' truth-content of *a* deriving from $A_T > B_T$ is other than a comparatively trivial by-product of the non-axiomatisability of the system now introduced.

Yet another limitation on the applicability of the theorem derives from the assumption that the competing hypotheses are related as antecedent to consequent in a deductive system. But in most cases of hypothesis-choice we are dealing with hypotheses not so related, as for example when a crucial experiment is set up whose logically possible outcomes x or $\sim x$ are such that x falsifies *a* and $\sim x$ falsifies *b*, or *a* entails $\sim x$ and *b* entails x. In applying Popper's theorem we can compare *a* with *b* only if *b* entails nothing that *a* does not also entail.

Two papers take up the question of empirical meaningfulness. Grover Maxwell reconsiders 'Criteria of Meaning and Demarcation', and argues that the search for clear-cut criteria either of the Carnap or Popper kind is bound to be frustrated. Maxwell has most sympathy with Carnap's 1956 proposals, which allow sentences to be significant if their terms are significant, but points out that theory-dependent postulates may be required to delimit meaningless terms, thus begging the question of the meaning of the theories providing these postulates.

In 'Verifiability and Logic', Wesley Salmon points out usefully that verifiability criteria of cognitive meaning (for example Ayer's) have rarely been discussed in terms of *inductive support*. He proposes to replace the usual criteria in terms of deduction of observation statements by 'a statement which is neither analytic nor self-contradictory is empirically verifiable if and only if it is either an observation-statement or the conclusion of a correct inductive or deductive argument from verifiable premises' (p. 360). This criterion calls for development of a satisfactory confirmation theory of empirical support. Without discussing this in general, Salmon makes some suggestions for dealing with particular difficulties, such as the non-transitivity of inductive support, and the vacuous apparent support given to meaningless sentences conjoined or disjoined with empirical sentences. It would have been useful to have had more discussion of the relation between empirical meaning and support, for if 'support' is construed as some function of increase of posterior confirmation, it seems that the criterion of meaning could be extended to include *dis*-support, in other words, anything is empirically meaningful to which observation statements have *relevance*, either positive or negative. It might also be remarked that his criterion is intended by Salmon to apply to *sentences*, not *terms*, and so does not seem applicable to the problem of the 'meaning of theoretical terms' to which the original verifiability criterion was meant to apply. There are difficulties about using a confirmation theory to explicate the meaning of such terms, because the question is in a sense already settled by admitting them into the language-basis of the confirmation theory. Furthermore, the problem of vacuous confirmations becomes pressing, because any theory yielding observably verified entailments is given greater than prior probability in a probabilistic confirmation theory, but many fictional theories can be invented to satisfy this condition without thereby acquiring 'meaning' in any acceptable sense.

In a substantial paper 'The Falsifiability of a Component of a Theoretical System', Adolf Grünbaum returns to the question of Duhemian conventionalism. He construes Duhem's thesis (*D*) in two parts: (1) experiment can falsify only a group of hypotheses together, not individual components of a theoretical system, (2) (claimed additionally by Quine) any one of the components could be preserved at the cost of regarding others as falsified. Grünbaum claims that (2) is true only in trivial cases, for example where the usage of words in an apparently falsified hypothesis is changed. The strong thesis that (2) is false in *all* non-trivial cases is not argued, since Grünbaum does not attempt to give the sufficient criteria for non-triviality which would be required to show this. Moreover, it is not clear either here or in his subsequent geometrical example that 'semantic stability' *is* always required when a hypothesis is non-trivially saved in face of undermining evidence. The law of conservation of momentum is in a sense saved in relativistic mechanics, and yet the usage of 'mass' is changed—it becomes a function of velocity instead of a constant property. But further argument along these lines is idle without more detailed analysis of what it is for a hypothesis to be the 'same', and what is involved in 'semantic stability'.

Grünbaum goes on to argue the weaker thesis that in non-trivial forms *D* is a non-sequitur, and that though falsification of *H* may involve a network of other hypotheses, say *A*, it still has to be shown separately that if *H.A* is falsified by *O*, there exists an *A'* such that *H.A'* is not falsified by *O*. He claims further that there are examples where *A* can independently be shown to be true, and hence where *A'* does not exist and *H* is independently falsifiable. His example is Einstein's assertion than any metric geometry can be preserved in the face of any empirical evidence. Here the auxiliary hypothesis *A* is the assumption that the spacial region is free from substance-specific deforming influences. Grünbaum continues with an argument for the independent truth of *A* which depends (*a*) on there being no *evidence* for an alternative *A'*, and (*b*) on the assertion that if *A* is false, many observation statements must be held to be false. But (*a*) is not conclusive for the truth of *A* (there is no conclusive evidence *against A'* either), and neither is (*b*). *A* is not itself an observation statement, and it is not clear that its falsity entails the falsity of any observation statement, for it is itself involved in a network of hypotheses which would be required in order to derive from it an observation statement. To show that the falsity of a particular hypothesis entails the falsity of a particular observation statement requires an investigation similar to the one required to refute *D*, and the success of such an investigation cannot be assumed in the attempted refutation of *D*. Grünbaum's next line of defence for the truth of *A* is the claim that if the truth of even observation statements is questioned, then no observation statements can falsify even a whole theory. But this argument begs the question also, for if falsification as well as confirmation is non-decisive, as *D* suggests, it may nevertheless be possible to hold that some observation statements relatively disconfirm a group of hypotheses, and others relatively confirm it, without 'the absurdity that any total theoretical system can be espoused as true a priori' (p. 288).

Finally, Grünbaum admits that these arguments show that '*A* is only more or less highly confirmed' (p. 289)—a significant retreat from the claim of truth. He thinks nevertheless that it is crucial that confirmation of *A* can be *separated* from that of *H*. But surely this is not sufficient for his purpose. For as long as it is not empirically demonstrable, but only likely, that *A* is true, it will always be possible to reject *A* in order to save *H*, and may in some circumstances be desirable to do so. Which is where the Duhem thesis came in. It must be concluded that *D* still withstands Grünbaum's assaults, and it may be conjectured that the question cannot be profitably pursued further unless greater care is taken over the criteria for identity of hypotheses.

Henry B. Veatch introduces the only theological note in the collection in 'A Case for Transempirical and Supernaturalistic Knowledge Claims'. He considers and rejects two objections to the logic of the cosmological argument: (i) it is essentially dependent on the ontological argument, since the only way we can construe its conclusion 'There exists a necessary being' is as ' "God exists" is a necessary proposition', and this is the conclusion of the o.a.; and (ii) the c.a. is an argument of the same kind as that for any transcendent scientific hypothesis, but its conclusion is neither verifiable nor falsifiable, hence as an argument of that kind it fails. Veatch has no difficulty in disposing of (i) by showing that the c.a. was never taken to imply a necessary ***truth***, only a necessary being; its conclusion was in fact stated by Aquinas to be a *contingent* truth. His disposal of (ii) is less happy. Having shown, correctly, that the c.a. differs from an argument to scientific theory in deducing the unobserved *conclusion* from the observed data, instead of the other way round as in the hypothetico-deductive construal of theories, he concludes that the c.a. is a simple modus ponens. It does, however, reach a conclusion about the unobserved, and if scientists are allowed to conclude to the unobserved, why not theologians? This, however, is less than cogent. Having shown that the paralled with the deductive method of science is not close, it cannot be appealed to when convenient to justify reference to the unobserved. It may be doubted too whether the modus ponens is as simple as all that. Certainly necessary beings do not occur in the evidence, and without some further a priori postulate it is difficult to see how such things can be reached in a deductive conclusion. But Veatch does not spell out the c.a. in sufficient detail to enable us to see what this further a priori postulate is or how we could know it to be true.

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Part III: *Philosophy of the Physical Sciences*

Part III of this volume is devoted to the philosophy of the physical sciences and contains contributions from Paul K. Feyerabend, Norwood Russell Hanson, E. L. Hill, Henryk Mehlberg and Wolfgang Yourgrau. It is a varied lot both with regard to quality and content. The latter is particularly unfortunate, for the connection between classical and quantum mechanics explored here by Hill has also been treated elsewhere by Feyerabend, Hanson and Mehlberg, and the question of the equivalence of theories which Hanson treats is pertinent to both the Feyerabend and Mehlberg papers but not discussed there.

Feyerabend has contributed a short note, 'On the Possibility of a *perpetuum mobile* of the Second Kind', which he suggests is to be read as an addendum to a short note of Popper's (this *Journal*, 8 (1957), 151). Feyerabend continues Popper's criticism of the introduction of negentropy as a way of saving the second law of thermodynamics from perpetual and systematic violations. His points here are well taken, that the concept of 'information' is introduced only by presupposing the second law, and that as subsequently developed information passes from an objective to a subjective sense. One could wish, however, that he had gone a bit slower and chosen to analyse the situation in greater detail. The fact that certain investigators have sinned (and surely Popper and Feyerabend are quite correct about it) hardly shows that Szilard's original programme for saving the second law is in error. Szilard's idea, say in the case of Maxwell's demon, was to consider the total entropy (in the thermodynamic sense) of the gas-plus-demon and show that as the entropy of the gas decreases that of the demon increases in such a way that the net entropy change for the combined system is positive (L. Szilard, *Z. Physik*, 53 (1929), 840). But perhaps one should not complain about Feyerabend's snap criticisms for he follows them up with nothing less than the design of a machine which does, he claims, systematically and perpetually violate the second law. Unfortunately here too there is little analysis. Just how and where are the violations supposed to occur? In what closed system does entropy continually decrease? Since the operation of the machine depends on the perpetual intervention of an outside observer who does expend work (he keeps putting sticks on supporting forks) it is not clear that the machine would violate any laws. Nevertheless, this is a lively discussion of a topic long overdue for critical attention. It is a sparkling example of Feyerabend's style and we only wish that, in this instance, he had chosen to write at greater length and in more detail.

The late N. R. Hanson has contributed a paper, 'Equivalence: the Paradox of Theoretical Analysis' that covers, with a few revisions and emendations, the same ground as his previous publications on the topic. Indeed, although no notice is given, this very paper has already been published (*Australasian J. of Phil.* 41 (1963), 217). The aim of the paper is to clarify and to point up the significance of the 'equivalence 'of physical theories. To this end Hanson has adopted wholesale the analysis of theories as algorithms plus physical interpretations. (Actually he says algorithms plus correspondence rules plus physical interpretations, but does not consider the correspondence rules.) By tabulating the four possible relations for a pair of theories two kinds of equivalence are uncovered: *strict* (logical, mathematical, formal) *equivalence* which obtains when the algorithms are mutually entailing and the physical interpretations are operationally equivalent and *operational equivalence* when, despite non-mutually entailing algorithms, the physical interpretations are still operationally equivalent.

The significance of strict equivalence is that (in accordance with Wittgenstein's early views on propositions) it is really identity. This identity thesis is maintained against Hanson's own earlier position to the contrary and despite at least ten (my own count) possible ways of construing strict equivalence as non-identity that are mentioned but not discussed. No argument is offered in support of the identity thesis although it seems to be connected with the use of entailment. Presumably entailment (as opposed to implication) is concerned with the meanings of the entities that enter into the relation and this, one may suppose, is how the identity thesis might emerge. But Hanson relates either pieces of algorithms or whole algorithms by entailment, and algorithms, since they require physical interpretation, must themselves be merely formal mechanisms, devoid somehow of 'physical meaning'. Why then speak of entailment here? For algorithms, even material implication will suffice. How, therefore, does identity creep in?

The significance of operational equivalence is not mentioned, although the concept is further explained as follows: the claim-content of the observational consequences of the theories will be (is?) the same. The paper, however, does not go on to explain just what the claim content is, nor how it happens that algorithmically distinct theories can be compared with regard to claim content, nor what is the distinguishing feature of 'observational' consequences. The use of this latter notion is particularly puzzling. The eighteenth-century wave and particle theories of light are cited as an example of observationally equivalent theories, despite their different consequences for the change in the velocity of light in passing from a less to a more dense medium. According to Hanson this consequence is not observational because it was not testable, and it was not testable because no one, then, knew what an experiment to test it would be like. Does this mean that no-one knew how actually to design such an experiment, or that scientists could not have recognised if given, say, the plans for Foucault's apparatus that it would test the consequence, or that measuring the speed of light was inconceivable in the eighteenth century, or what? In the case of the Tychonic and Copernican systems of astronomy it is claimed that because different orbits of Mars are 'potentially observable' on the different theories, the theories are not observationally equivalent. Could one not say that different velocities of light are likewise potentially observable? It seems that either the case for the theories of light is in error or the case for the astronomical theories is incomplete, perhaps both.

The conclusion on the astronomical theories, also mentioned are Ptolemy *v.* Copernicus, is that these are cases of 'restricted observational equivalence'; i.e. of observational equivalence with respect to a common proper subset of the observational consequences of the theories. Since apparently no difficulty with using this notion is noticed, perhaps it is in order to point out that *any* two theories are so equivalent. For given two theories let the subset consist of whatever 'observational consequences' the theories have in common or, if there are none of these, the null set. With regard to this subset, every observational consequence of one theory is an observational consequence of the other.

As in much of his work, Hanson has here seen the importance of a topic not well treated in the literature and has treated it in an original and interesting way. The critical points that I have raised only show the stimulation of his effort.

E. L. Hill, 'Classical Mechanics as a Limiting Form of Quantum Mechanics', discusses some of the connections between these two theories. Along the way he presents a simple space-time version of non-relativistic quantum theory, makes some interesting remarks on the derivation of the uncertainty formulae and offers an instructive view of Hamilton-Jacobi dynamics. His basic contention is that classical mechanics (CM) is not a limiting case of quantum mechanics (QM) and he argues this position both for kinematics and dynamics.

If one takes QM seriously, as a complete theory, then one must deny the existence of the positions and motions of particles in the sense of classical kinematics. The reason, according to Hill, is that QM does not provide a technique for defining these concepts. Hill even rejects Heisenberg's restricted use of the concepts: position and momentum can each apply separately but not simultaneously. His arguments here are not at all clear. He cannot actually be concerned about what can be defined, since that depends mostly on the linguistic formulation of the theory. He may have in mind von Neumann's demonstration that continuous quantities, like position, cannot even in principle be measured exactly. (In the traditional way for writings on QM, Hill scarcely differentiates what *is*, from what can be *defined*, from what can be *measured*.) Thus one cannot make a straightforward identification of the classical with the quantum concepts. It does not follow, however, that one cannot make such an identification *under restricted conditions* or that one could not, under suitable restrictions, identify the classical concepts with certain functions of the quantum ones. For example, the conclusion on exact measurement breaks down for bounded systems and one might suppose that any classical system is inside a big enough box; or for sharply localised wave functions one might identify, say, classical position with the mean value of the QM position operator. Hill sometimes seems to forget that it is only under such restrictions that QM is supposed to yield CM; no-one has ever claimed their unrestricted equivalence.

From kinematics Hill passes to dynamics and the attempt to derive the classical laws of motion. He considers the attempt to get Newton's laws for quantum systems with relatively large kinetic energy and mass, via Ehrenfest's results, and the attempt to get the Hamilton-Jacobi equation by neglecting Planck's constant. He rejects both attempts on the basis of his previous conclusion that the concepts involved must differ from the classical ones and, therefore, the formulas even if obtained could not relate the classical quantities. He supplements this with a different, special objection for each case. He objects that the wave packet spreads in time, for the Ehrenfest case, and that the Hamilton-Jacobi equation does not admit linear combinations of solutions whereas the QM counterpart does. Thus he concludes that 'there exist strong mathematical analogies between the two theories, but at no point do we find real equivalence' (p. 447).

Hill's exposition throughout is very lucid and I much recommend it. I cannot help feeling that had he paid somewhat greater attention to what the claim that CM is a 'limiting form' of QM amounts to, he would have reached an entirely different conclusion. Surely the claim is that under suitable restrictions formulated in QM ('limiting' conditions) there are concepts in QM which are surrogates for the basic concepts of CM in the sense that when applied to objects suitable for treatment by CM they yield results identical with the classical ones within suitable degrees of approximation (which can be specified), and that these concepts enter into a system of laws, derivable in QM under the given restrictions, that are formally the same as the laws for the classical concepts. So understood, I think Hill's own expert resumé of the technicalities would show that indeed CM is a limiting form of QM.

Unfortunately it is not possible here to do an adequate review of H. Mehlberg's 'Relativity and the Atom'. This paper is the longest and most substantial of Part III. It is both too detailed and too shallow, for Mehlberg probes a score of pertinent topics but seldom has sufficient space to give them their due. I hope that his work, here and in the companion paper that he cites, will be expanded to book length. It should be a book well worth reading.

The primary task of Mehlberg's paper is to prepare, present and discuss a remarkable, physically pregnant axiomatic system for the four dimensional world geometry of special relativity. Because Mehlberg holds that contemporary applications in quantum theory make untenable the conventional interpretation of relativity in terms of rigid bodies and clocks (is there some verificationist position here?) he wants his system to be suitable for both macro and micro domains. Because spatio-temporal coincidence, which is a dominant concept of relativity, can be reduced to particle collisions which are dominant in quantum theory (S-matrix, theory of measurement, etc.), Mehlberg bases his system on a concept of collision. The actual concept chosen is, moreover, time symmetric and thus reflects the time symmetry of what Mehlberg calls 'cosmic laws'. He claims to have a 'watertight' explication of 'cosmic law' to be given in a forthcoming paper and based, apparently, on the system presented here and on some unnamed meta-mathematical results.

The axiomatic system is developed in terms of a single symmetric, binary relation 'collision-connectibility' that holds between pairs of events. In twelve ingenious definitions Mehlberg gets from this alone the concepts of spacelike betweenness, collinearity, inertial frame, timelike instant, spatial, temporal and spatio-temporal coincidence, spatial and temporal congruence, Euclidicity and others. There are eight axioms and six theorems are cited, without proof. Among the theorems are those showing that the Lorentz transformations and only these relate co-ordinate systems associated with inertial frames. This result culminates a lengthy and fascinating discussion of attempts to derive the Lorentz transformations by Einstein, Weyl, Carathéodory, Pauli and others. Mehlberg's discussion here displays considerable analytical skill and among the byproducts is a distinction between the principle of relativity and the 'principle of reciprocity' (each law relating frame K to frame K′ has a companion relating in the same way K′ to K). Pauli is charged with using reciprocity while assuming only relativity and I think the same charge holds for the fashionable use of the 'K-calculus' in developing special relativity (cf. D. Bohm, *The Special Theory of Relativity* (W. A. Benjamin, New York: 1965), chap. 16). Mehlberg also provides an interesting classification of physical laws that depends on their expressibility within a single frame of reference.

The remainder of the paper considers some implications of the system for time: the system establishes the 'nondirectionality' of time, when 'cosmic law' is properly given one can even get temporal isotropy—yes, Mehlberg knows about the meson decay experiments which may force a breakdown of time symmetry via the TCP theorem—and the system shows that relativistic time is incompatible with any relational theory (would that his remarks were clearer here). Finally various metamathematical issues relevant to the axiomatisation of any physical theory are examined.

This paper ought to serve as a case study for those philosophers of science who tend to deprecate the value of axiomatisations. The variety of issues impinged on in order to formulate the system, the economical formulation itself and the consequences which emerge add up to real advance in our understanding of relativistic space-time. To be sure there are critical points. The style is sometimes oblique and the exposition is not uniformly clear. The logical (or set-theoretic) setting for the axiomatic system is not given. Clearly the system presupposes some temporal notions, for 'collision-connectibility' relates events and these are happenings in time. How are these presuppositions related to the temporal consequences drawn from the system? Critical points and all, however, this is a *tour de force* and a work of major importance.

ARTHUR I. FINE
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Rudolph Carnap : *The Logical Structure of the World, and Pseudoproblems in Philosophy*. Translated by Rolf A. George (London, Routledge and Kegan Paul, 1967, 60*s*.) Pp. xxvi+364.

Der logische Aufbau der Welt was Carnap's first considerable work. It was planned and written in the years immediately after 1921, when he finished his studies under Frege at Jena, but was not printed until 1928, two years after he had settled in Vienna as a member of the Circle organised by Schlick. That it now appears in English for the first time, forty years after its composition, is perhaps some evidence of Carnap's standing among English-speaking philosophers. Few who have read the original German have been convinced by his positivistic system building, even though themselves admirers of Russell and Wittgenstein from whom he professed to derive his main ideas. But the influence of his other writings has been so great that there is a natural wish to have all his works available in English, and this publication will undoubtedly make it easier to appreciate his place in the positivist movement. The translator's work was not easy, but apart from a few small slips, such as using 'logistics' (which is a word of French origin for the quarter-master's craft) in place of 'logistic', he has done his work well. The reader may sometimes suffer from a feeling of mental indigestion when he has had the bad luck to come on a number of sentences such as 'The epistemological nucleus of every concrete cognition of heteropsychological occurrences consists of a perception of physical phenomena' (p. 316). But it is difficult to see how a conscientious interpreter could have written anything else in these passages. Sometimes, when he has had special difficulty in finding a suitable English term, the translator gives the original German in a footnote. In two or three places he might perhaps have allowed himself a little more liberty of comment, as for example on page 282 where 'phenomenalism' is used for a variety of Kantianism rather than for what is commonly understood by that name in English.

In a new preface, written for a reprint of the German text in 1961, Carnap says that he still agrees 'with the philosophical orientation which stands behind this book', i.e. with the attempt to provide a 'rational reconstruction of the concepts of all fields of knowledge on the basis of concepts that refer to the immediately given'. But he goes on to add that he has come to think it somewhat artificial to try to base all on the single concept he formerly used, namely recollection of similarity, considered as a relation between elementary experiences in the sense in which *Gestalt* psychologists might use that phrase. He would 'now consider for use as basic elements . . . something similar to Mach's elements, e.g. concrete sense-data' and would 'choose as basic concepts some of the relations between such elements, for example "x is earlier than y", the relation of proximity in the visual field and in other sensory fields, and the relation of qualitative similarity'. And although he still holds that 'the positivist thesis of the reducibility of thing concepts to autopsychological concepts remains valid', he does not now wish to say that 'all statements about things can be *translated* into statements about sense-data', since he how thinks the desired reductions can be achieved only by use of 'more liberal forms of concept introduction', in particular the reduction sentences about which he wrote in his *Testability and Meaning* of 1936. Similar reservations are required also in relation to other reductionist (or constructionist) theses, including that of physicalism which he developed in the thirties with Otto

Neurath. In fact he is now inclined to think that the theoretical concepts of the advanced sciences and 'all concepts referring to heteropsychological objects, whether they occur in scientific psychology or in everyday life', can never be explicitly defined because they are introduced 'through theoretical postulates and correspondence rules' which do not allow for a complete interpretation by reference to observation terms.

What is left of the reductionist programme after all these concessions? Not much. But there was always something strangely thin and unsatisfactory about Carnap's notion of reduction. At the beginning of his intellectual career he was convinced by the thesis of extensionality which had been put forward by Wittgenstein in his *Tractatus* (with special reference to propositional signs) and accepted by Russell in the second edition of *Principia Mathematica* (with special reference to signs for propositional functions). When he wrote the *Aufbau* he thought indeed that he could prove it impossible to have a genuinely non-extensional statement about a propositional function (p. 77) and he was therefore prepared to dismiss as irrelevant any criticism of his 'rational reconstructions' which was based on the objection that he had not respected the *meanings* of the statements made in ordinary life. According to his theory it was sufficient to provide a 'logical translation' for each respectable sentence of natural language, i.e. a translation that preserved the truth-value, though not necessarily the sense, of its original (p. 85). It was this notion of translation he had in mind when he suggested already in 1928 (i.e. before his collaboration with Neurath) that, just as every statement about physical objects could be translated into a statement about psychological objects, so too every statement about psychological objects might be translated into a statement about physical objects (p. 92). In his view all that would be required for such a physicalist reduction would be detailed knowledge of a thorough-going and universal correlation between mental processes and brain processes, and although he admitted that it was not yet possible to formulate such a correlation he was quite prepared to assume its existence. To ask for more than bare correlation was, he thought, to raise a bogus problem about the 'essence' of the relation between mind and body—a problem which had come to be the central topic of metaphysical delusion (p. 37).

It is a mark of Carnap's intellectual honesty that he soon came to have a bad conscience about the thesis of extensionality and has never ceased to be worried about it. Already in *The Logical Syntax of Language* he rejected Wittgenstein's dogma that there cannot be an intensional language in favour of the weaker assertion that everything can be expressed in an extensional language, and he has maintained ever since that the weaker assertion is no more than an unrefuted hypothesis. Here in his new preface of 1961 he points out that for reconstruction of a given concept A through concept B it is certainly not sufficient that B have the same extension as A, since 'the co-extensiveness of A and B must not be accidental but necessary, i.e. must rest either on the basis of logical rules or on the basis of natural laws'. But after all allowance has been made for such reservations the thesis remains strange and produces curious results. When, for example, Carnap comes to that stage in his rational reconstruction where he should show the possibility of constructing physical objects from what he calls autopsychological objects, he is content with rather vague talk about 'the assignment of sense qualities to world points' and pays no attention at all to what since Berkeley's time have been regarded as the most important problems of the kind of construction he seems to promise, namely accounting for the relative permanence and the causal powers which are ordinarily supposed to belong to physical objects but not to sense-data.

No doubt Carnap might defend himself by saying that his purpose was utterly different from Berkeley's, but I strongly suspect that some of the fascination he finds in reductionism depends on his reading into his reduction formulae not only more than is justified by his arguments but more than could possibly be justified by any arguments consistent with his general principles. Certainly his proffered reduction of cultural objects to heteropsychological, heteropsychological to physical, and physical to autopsychological falls far short of the step-wise arithmetisation of mathematical analysis with which he likes to compare it. Indeed of the three steps required only that from cultural to heteropsychological is made at all plausible. Perhaps it was no accident that in the hey-day of philosophical talk about logical construction our favourite example was the construction of nations from individual men.

At the beginning of his first chapter Carnap quotes as a motto Russell's remark 'The supreme maxim in scientific philosophizing is this: Wherever possible logical constructions are to be substituted for inferred entities'. Let us agree that new talk about imaginary numbers or the general will is not related to old talk about real numbers or the wants of natural persons as talk about America was related in the sixteenth century to talk about Europe nor yet as talk about cave men is related now to talk about our neighbours. But why should we assume that it must be possible to set out once and for all time a scheme for constructing all conceivable entities from given entities of one type? It looks as though one motive for making the assumption has been a strong desire to delimit the field of admissible speculation. Several times Carnap tells us that this or that problem is hopelessly metaphysical because it cannot even be posed within the bounds of his construction theory. He may perhaps be right in what he says about the limits of his theory and yet be wrong in his advice, much as Comte was wrong when he warned men of his time against bothering about the chemical constitution of the stars. By all means let us be tough-minded in detail with speculations as they occur, but let us not try to give men a *rule* for being tough-minded in all the intellectual situations in which they may hereafter find themselves. Positivism of that kind is related to dogmatic metaphysics in much the same way as fascism is related to communism.

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Richard Feynman: *The Character of Physical Law* (M.I.T. Press, 1967, $2.45). Pp. 173.

This book consists of a transcription compiled by BBC-2 from their tape, shown as part of the Further Education Scheme, of the Messenger Lectures delivered by Professor Feynman at Cornell University in 1964. Because the author has a well deserved reputation as an expositor and lecturer (not to mention his outstanding scientific accomplishments), one inevitably comes to this book with fairly high expectations. I am sorry to say, that like a too-much-recommended 'B' movie, one is likely to be disappointed. In part this is due to the fact of transcription, for what may have been full of life when delivered from the lecture platform to an enthusiastic audience, is here stillborn from the press. Perhaps, also, the material was stale to Feynman himself, for surely he has lectured before to lay audiences on the same topics and certainly in his writings (the *Lectures*, for example) many of the same illustrations are used to illustrate the same points. But these are not the only sources of disappointment. In a recent address (in *Supplements al Nouvo Cimento IV*, 492 (1966)) Feynman accused the scientific community of making science irrelevant, by failing to engage the lay public on issues to which science is relevant. In these lectures Feynman himself provides some evidence for the charge, by making no attempt to engage either contemporary historians or philosophers (even the 'tough-minded' ones) on issues relevant to understanding the development of physics.

The avowed purpose of these lectures is to discuss the general characteristics of physical laws. Feynman takes the law of gravitation as his key example. The treatment of its discovery is a paradigm of linear history: in the beginning were the ancients, then came Copernicus rediscovering the earth moving around the sun, then some debates, then Tycho Brahe who liked to collect data and worked hard, then Kepler the empirical generaliser, then Galileo the discoverer of the principle of inertia, finally Newton: $F = ma$, and gravitation. From this law several features common to all laws are set out: laws are mathematical, inexact (subject to correction), simple (in pattern, not in their action), and universal in scope. To this list Feynman later adds that laws are abstract and do not yield a direct path towards understanding nature. The mathematical character of laws leads to a general discussion of the uses of mathematics in physics. Feynman contrasts Babylonian (non-axiomatic) mathematics with Greek (axiomatic) mathematics and argues that physics must be in the Babylonian tradition because the validity of laws often turns out to extend beyond the range of particular derivations. Thus it is pointless to focus on the derivations and codify them in axiomatic systems. The actual procedure in physics, Feynman suggests, is to use some known laws to get, by derivation, some others and then to guess that these other hold even if the initial ones fail. Curiously, Feynman does not notice that this description also fits the process of abstraction in the development of axiomatised mathematical systems. The Babylonian-Greek contrast, therefore, would seem to mark no more than the old discovery-justification distinction. This point is reinforced by Feynman's view that the physicist must always keep several versions of the same structure in mind, for different versions may suggest different extensions ('guesses') and different versions may be impinged on differently by experimental results.

After these methodological reflections, the lectures go on to consider some important types of laws: conservation laws, symmetry laws, the law of entropy, and the probabilistic laws of quantum theory. In a 'just look at these great ideas' manner Feynman gives a popular exposition and discusses some interesting interconnections. Perhaps on the (probably correct) assumption that the audience for such lectures enjoys a bit of mystification, the presentation is intermittently obscure. (My candidate for honours here is the following excerpt from page 161, which is the entire discussion on the point in question: 'In the case of the continuous space, suppose the precise proposition is that space really consists of a series of dots, and that the space between them does not mean anything, and that the dots are in a cubic array. Then we can prove immediately that this is wrong. It does not work.') Some of his remarks offend against good sense; for example, in discussing irreversibility Feynman blandly asserts, in effect, that retrodictions are simply predictions of future documentation. He goes on to suggest the hypothesis that the past is more organised than the present, as necessary to account for the success of induction. The worst offences, however, are in the chapter on quantum theory. The discussion there is focused on the two slit experiment as a way of elucidating the use of probability amplitudes. Along the way, Feynman says that ordinary logic is subject to direct experimental test in quantum theory and that when so tested it is found wanting. The uncertainty principle, according to him, is equivalent to the impossibility of *designing* a certain sufficiently nice measuring apparatus. Hidden variable theories 'cannot be true'. He pokes fun at an anonymous philosopher who insists on a modicum of causality in science and concludes pompously that science must be free from 'pompous preconditions'. The total effect of this chapter is mystifying (as well as annoying) and Feynman seems content to sanction its unintelligibility as a 'deep mystery' (p. 145).

The final chapter of the book is devoted to heuristics. In science, we learn, the procedure is to make a guess, to draw out the consequences of the guess, and then to test them. Anything that aids good guessing—philosophy, models, ideals of simplicity or beauty—is sanctioned. Ultimately, however, it is only experimental tests that will tell. Feynman ends by speculating on the future of science: either, like the hare, scientists are doomed forever to be going only half of the way to discovering nature's laws, or else in time perhaps all the laws will be known. Either way the prospects are hellish. In the first case surely the race is tiring and one is bound to get bored. But should all the laws be known, worse still, for then, 'The philosophers who are always on the outside making stupid remarks will be able to close in, . . .'

ARTHUR I. FINE
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Richard S. Rudner: *Philosophy of Social Science* (Englewood Cliffs, N.J., U.S.A., Prentice-Hall, 1966, 14*s*.) Pp. x+118.

This is in many ways an admirable book. It is a beautifully written summary of one particular view of social science, and since it is a central argument of the book that, so far as logical and conceptual structures are concerned, social science in no important way differs from natural science, it is also a summary of one particular view of natural science. The reader of Braithwaite, Hempel and Nagel will find nothing to surprise him here, except perhaps Rudner's gift for unusually brief and elegant expositions. The main topics covered are the construction and structure of theories, the uses to which theoretical formulations may be put, the question of the relation of social science to natural science and the problems arising from attempts at teleological and, more specifically, functionalist explanation.

The book's defects are of three kinds. First, who will read it? The editors of the series of which the book forms a part apparently view it as a textbook and even perhaps an introductory textbook. But for this purpose it is much too compressed. Rudner himself says that one major aim is 'to provide the student of the literature of social theory with some bases for critically assessing the variety of theoretical formulations in that literature', but anyone who comes fresh from the literature of social theory and who is not already familiar with the philosophy of science will find Rudner's book very strange. This is because there is relatively so little space devoted to the conceptual problems which actually and presently preoccupy social theorists.

Secondly, Rudner is at his least convincing on just these topics. He misunderstands Peter Winch's arguments and exhibits no sense of the importance of the concepts of a rule for the philosophy of social science. On the other hand his discussion of teleological explanation is excellent and should—along with Nagel's discussion—be required reading for social theorists.

Thirdly, as so often in contemporary philosophy of science, there is an inability to discard empiricist and neopositivist trappings which are unnecessary to the argument. Rudner argues cogently for the view that we possess no satisfactory account of how empiricist canons can allow for scientific discourse about dispositional properties or theoretical entities. But he still appears to believe that somehow or other science must come to terms with empiricism. More seriously, in his account of the use of models he never marks the proper distinction between those models which are and remain heuristically useful formal analogues, but nothing more, and those whose applications leads on to the identification of some new class of entities which provide a new subject-matter for further theorising.

Yet to complain of what Rudner does not provide may be unfair. Within the constraints imposed by the length of his book it is marvellous how much he has achieved. What one must doubt is whether within such constraints a genuinely satisfactory book covering the same ground could be written.

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J. Merleau-Ponty: *Cosmologie du XX^E Siecle* (Paris, Gallimard, 1965). Pp. 533.

It now seems very likely that the year 1963 marks one of the great turning-points in the development of observational cosmology. For in that year the first quasars were discovered, and it rapidly became evident that if their red shifts are cosmological in origin then we would at last be able to see far enough in distance and long enough ago in time to discriminate between rival theories of the Universe. The evidence is not yet final but it strongly suggests that the steady state theory is wrong, and that the Universe as we know it has developed from an earlier quite different state. The present evidence is based on about two hundred quasars half of which have known red shifts, but in a few years there should be thousands known.

The future development of cosmology is likely to be fundamentally affected by these and other observational developments (such as the discovery made in the last two years that the universe is probably filled with black body radiation at a temperature of 3°K, the relic radiation of the 'hot big bang'). Thus for historical purposes the mid-1960s represent a natural culmination of one phase of cosmology, in which theorising went on in the absence of any facts except the great fact of the expansion itself. By chance the book under review is a historical account of cosmology up to 1962, without the author realising that quasars and relic radiation are about to transform the subject. Apart from its title, which reminds one very forcibly how unexpected were these discoveries, the book gives an admirably accurate and discerning account of cosmology up to 1962.

This is not a simple task because ideally all the contradictory theories should be expounded with sympathy, but there is something about the Universe which brings out all the latent bigotry in man, so that in defining his own attitude to it he must fiercely tend to exclude all other attitudes. (The writer must regretfully include himself among the guilty ones.) Against this charge Dr Merleau-Ponty can be acquitted. The different theories are all given their fair share of attention, and the author tries to emphasise what is best in each of them. His plan is the natural one of beginning with Einstein's first paper of 1917 which applied the field equations of general relativity to the universe as a whole, and following it with the relativistic developments we associate with the names of de Sitter, Friedman, Lemaitre, Eddington, and Robertson. Then comes a section on the 'heretical' theories: kinematic relativity, the steady state model, and the theories of Eddington, Dirac and Jordan. Finally he considers cosmogony, that is, the formation of objects inside the universe. A good deal of the basic mathematical development is given in a series of appendices—the text itself should be intelligible to non-mathematicians.

What is the overall impression the book makes? It is, I am afraid, that the attempts to find Grand Principles underlying the structure of the Universe have not been very successful. Cosmologists can now turn with relief away from such attempts, to a straightforward astrophysical analysis of all the new data that is pouring in. We might reasonably expect this astrophysical phase to last two or three decades. After that, perhaps, it will be time to turn back to Grand Principles, and then we will at least have a somewhat greater chance of success.

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J. B. Stallo: *The Concepts and Theories of Modern Physics*. Ed. Percy W. Bridgman (Harvard University Press; Oxford University Press, 1966. 16*s*.) Pp. xxix+325.

This book is published as part of the John Harvard Library which specialises in otherwise inaccessible source material from the American past. It consists of a reproduction of the third, 1888, edition of Stallo's original work, usefully prefaced by twenty-three introductory pages from the noted physicist and philosopher of science, P. W. Bridgman.

As Bridgman points out, at the time of the original publication the subject matter was quite novel to most of Stallo's readers and this undoubtedly accounts for some of the misguided criticism of it referred to in Stallo's preface to the second edition. In the first sentence of the book Stallo makes it clear that it is intended as 'a contribution, not to physics, nor certainly to metaphysics, but to the new theory of cognition.' Whilst this reference to the 'new theory of cognition' needs expansion, we can agree with Bridgman that the book 'today . . . would unhesitatingly be classified as philosophy of science'.

Nevertheless, its interest for modern readers probably lies more in the history of science. On the one hand it gives a good description of the conceptual framework adopted by the majority of the physicists of the time, supported by extensive quotations from their leading spokesmen. It also presents a rudimentary philosophy of science with features that were to be found later expanded into the positivist and energeticist schools associated with the names of Mach and Ostwald at the turn of the century. Indeed Mach was instrumental in the publication of a German translation of Stallo's book to which he added a thirteen-page introduction.

Unfortunately, Stallo leaves the statement of his philosophical thesis till a good way through the book. The reader is plunged straight into a caustically cynical attack on the 'atomo-mechanical' theory in which each of its major tenets is methodically reduced to scientific and logical absurdity. By the end of the eighth chapter one is left with the impression that Stallo's sole aim is to carry through a one-man vendetta against an assumption which was basic to the physical world-view of most of his scientific contemporaries.

In the preface to the second edition, however, Stallo assures his critics that this is not the case. The real motive for his work is disclosed in chapters 9-12, but was left to this late stage so that readers would not be unduly deterred by the abstractions of philosophy at the start. In fact Stallo later recognised that this back-to-front layout probably caused more confusion than it avoided and was largely responsible for the misapprehensions of his critics.

It must be borne in mind, therefore, that the several theories which come under such merciless scrutiny in Stallo's book are meant only to be taken as illustrations of his central thesis—that the process of cognition leads us to the adoption of metaphysical fallacies.

According to Stallo, the atomo-mechanical theory is essentially metaphysical since it fails to fulfil two necessary conditions of valid scientific hypothesis. These are (1) that the hypothesis should provide a link between 'a fact to be explained and a fact by which it is to be explained' so that the number of unexplained facts is reduced by at least one, and (2) that the explaining fact should be a datum of experience.

But how, when physicists thought that metaphysics had been eliminated with the advent of Newton, did metaphysics remain? This is what Stallo explains on the basis of the 'new theory of cognition'.

Stallo maintains that the fundamental error is a confusion between 'thoughts and things' arising from the fact that entities of 'objective reality' are manipulated in the mind in the form of concepts. Concepts and the 'things' they designate are therefore thought to behave in similar manner. This basic confusion, claims Stallo, leads to the four errors of metaphysics listed on pages 159-160 and was responsible for the untenable assumptions of the physics of his day.

Thus the popular 'nebular hypothesis' of cosmogony arose from a desire to derive the multiple from the simple in the world of objects following the derivation of the specific (detailed) from the general in the world of concepts. Non-Euclidean geometries are then seen as a metaphysical reification of concepts carried out to reconcile such 'absurd' cosmological theories with the scientifically invalid atomo-mechanical theory.

The vindication of many features of the atomo-mechanical theory in present day atomic physics and of non-Euclidean geometry in General Relativity gives the scornful prose of Stallo's critical passages an ironic quality, but the force with which his arguments still come over is a tribute to his career as an advocate. Perhaps, as Bridgman says, Stallo should have paid more attention to what scientists do than to what they say they do.

The book is not wholly negative, however. The concluding chapter contains, among other things, a discussion of thermodynamics and a presentation of the belief in energy as a basic explanatory principle. The chapter on cosmology includes a sketch of the theory of meteoric agglomeration with which one feels Stallo had more sympathy than he held for the nebular hypothesis. There is also Stallo's attempt to deal with the 'problem of demarcation' by giving positive criteria for the distinction between physics and metaphysics.

Above all, Stallo's book is an early landmark in American philosophy of science and, whilst it is an important source for historians, it is also well worth perusal by philosophers of science. The present publication makes this very much easier.

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H. A. Nielsen: *Methods of Natural Science* (London: Prentice-Hall International, 1967, 14*s*.) Pp. xv+70.

We have here an introduction to the philosophy of science only sixty-seven pages long, and intended for the beginner in this field. Now there seem to me to be two requirements which such a book ought to satisfy. First it should introduce the reader to some at least of the current problems in the field and second it should provide him with references enabling him to investigate these problems further. Unfortunately, however, Mr Nielsen's book does neither of these things.

Let us take for example the chapter on 'The Logic of Explanations'. One would expect an account of the deductive model of explanation and references to some of the criticisms of it which have been advanced recently, e.g. those of Feyeraband and Scriven. But the chapter contains no such account and no such references. Indeed Mr Nielsen is guilty of a number of errors which appear to result from a failure to understand the deductive model properly. On page 19 he makes the remarkable statement: 'the positivistic demand for testability of hypotheses as a condition for admitting them into meaningful discourse has influenced the greater part of recent discussion on scientific method.' But surely it is well-known that the positivists advanced the *verifiability* criterion of meaningfulness and that verifiability and testability are not the same. How then did Mr Nielsen come to confuse them?

The answer I think is this. According to the deductive model we explain a statement S by means of a universal law L and certain initial conditions C such that $C \,.\, L \vdash S$. In some cases, e.g. Geology it may be more of a problem to find C than L. In others, e.g. theoretical physics vice versa. Mr Nielsen considers only examples of the former type. Hypotheses for him are then singular statements of initial conditions C, or, as he puts it on page 20: 'Most philosophers of science, including positivists, would agree that when we ask for an explanation of a puzzling physical fact, we are asking for a reference to some efficacious physical agency capable of producing that fact.' His illuminating example is of explaining a puzzling hole in a bread wrapper by postulating that a mouse chewed it. Since an explanation is accordingly expressed by a singular statement we can see how testing an explanation might be regarded as verifying it. At all events whether or not Mr Nielsen's failure to expound the deductive model is connected with his mistaken account of the positivists' position, he is certainly guilty on both counts.

The situation is similar in chapter 3 which, although about 'Laws and General Theories', contains no mention of the dispute between realists and instrumentalists or of the problem of counter-factuals. Chapter 5 on the 'Nature and Limits of Scientific Truth' is perhaps more interesting. Here Mr Nielsen distinguishes two spheres or, to put it another way, two classes of propositions. On the one hand we have scientific truths. On the other (p. 52): 'The concept of truth relates to still another class of propositions that may be described in general as truths about the individual existing man. We can call these "Socratic truths". . . .' The purpose of this distinction is to show that there can be no clash between science and religion since they belong to different spheres. Thus Mr Nielsen does not see any contradiction between the Genesis account of the creation and the findings of modern science. He tells us on page 59 that 'we should not be diverted by the fact that some statements in which the idea of creation appears read like plain cosmogony, biology, or natural history', and explains on the next page that '. . . the sentences in Genesis, that have "whales" and "fowl" as their grammatical subjects claim to be addressing an understood subject, the reader himself in his creaturehood.' This unhistorical procedure of reinterpreting the sentences of Genesis so that we can still assign to them the truth-value 'true' is hardly very satisfactory. Altogether I do not think that Mr Nielsen's neo-fundamentalist position is at all tenable.

As well as dealing with contemporary issues, Mr Nielsen has a historical chapter expounding in twelve pages the views of Aristotle, Bacon, Descartes and Mill on philosophy of science. Though extremely cursory, this is perhaps the best part of a book which has little to recommend it.

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George A. Miller: *The Psychology of Communication* (New York: Basic Books, 1967, $4.95). Pp. vii+197.

This book by the eminent Harvard psychologist should be a treat to any philosopher, for it discusses some interesting problems that used to be classed as philosophical and it does it with originality, depth, clarity, and wit, as well as with an uncommon awareness of the methodological issues involved. The seven essays were written over the last ten years. All but the last have appeared before, either in scientific publications or in general journals like *Encounter*.

'Information and memory' shows that what we can recall depends essentially on the number of items presented and on their organisation. Hence it pays to store those items that contain the largest amount of information and at the same time fit together: after all, unlike rats, we can proceed rationally. Any given text should then be assigned two different measures: its physical length (the number of symbols it contains) and its psychological length (the number of ideas and their organization). Which argues against a purely behaviourist account of memory, for at the highest level we cannot disregard the ideas the symbols stand for. A byproduct for the technology of teaching: teach theories (preferably axiom systems) rather than heaps of isolated bits of information that are forgotten as soon as the exams are over.

The next two essays—the classic and amusing 'The magical number seven, plus or minus two', and 'The human link in communication systems'—investigate man's ability to process information. They discuss numerous experiments designed to measure the channel capacity of human observers—i.e. the greatest amount of information subjects can give us about a given set of stimuli presented to them. The results are depressing: if we focus on a single property, such as sound loudness, we will not be able to form more than half a dozen different categories—unless we have had a special musical training. It is only by combining several variables—e.g. sound frequency, intensity, duration, and spatial location—that we can improve our channel capacity—but then at the price of diminished accuracy. We prove to be miserable communication channels: 'Compared to telephone or television channels, man is better characterised as a bottleneck.' Our 'only' advantage over the telephone and the telly consists in discovering new ways to transform the information we receive—e.g. in being able to invent the telephone and the telly. All this can be made precise by means of information theory. Moreover, many of these experiments on our performance as information channels would not have been designed were it not for this theory, and many old questions concerning our ability to recognise and communicate could have received no precise answers without the help of that theory. One more setback to both prescientific perception theory and to the observe-then-think philosophy.

'Concerning psychical research' is a delightful review of C. D. Broad's *Lectures on Physical Research* (1962). Miller complains about Broad's methodological weakness—as when he neglects the effects of optional stopping (hence selective sampling), or when he takes for granted the integrity of his witnesses and the accuracy of their memories, or when he (in line with Rhine) interprets improbability as paranormality. The verdict is clear: nothing has been proven, so that the parapsychologist's claim 'is fraudulent. An innocent fraud, perhaps, but no less vicious for its good intentions.' Friends of the ghostly, whether in Greenland or in Britain: take note.

The rest of the book, which is nearly two-thirds of it, is concerned with psycholinguistics. Some of the questions concerning the relation of speech to thought and the psychological (or even biological) basis of grammars were posed a couple of millenia ago. In particular Wittgenstein and his followers have dealt with some psycholinguistic problems (though they would claim they were philosophical questions), such as how do we learn a language and, in particular, how do we disclose its grammatical rules. But, of course, Miller handles these problems as a scientist, not as an amateur: he theorises and he subjects his theories to experiments. Indeed, it was largely through the efforts of Noam Chomsky and George A. Miller that psycholinguistics was born as a scientific discipline some ten years ago.

As conceived by Chomsky, Miller and their associates, psycholinguistics is not limited to 'observing the verbal behaviour of human subjects': 'the central task of this new science is to describe the psychological processes that go on when people use sentences' and, in particular, when people grope their way to discovering the patterns of a new language they try to learn. The author's main theses are these. First, a language is not a game with arbitrary rules: the mere phrase 'language game' exhibits a superficial approach. A language is a communication tool that has a biological basis—so much so that 'grammatical sentences are much easier to hear, utter, or remember than are ungrammatical strings of words, and even nonsense (*pirot*, *karol*, *elat*, etc.) is easier to deal with if it looks grammatical (*pirots karolize elatically*, etc.)'. Second, the behaviourist accounts of language learning are empirically false: experience fails to show a gradual improvement of performance with reinforcement. Moreover, the behaviourist approach is barren, for it does not ask the most important questions, such as what goes on in our minds when we try to find out the pattern (the set of 'rules') a given set of symbols obey. Third, the psycholinguist should avail himself of two new basic tools in order to investigate his problems: information theory and the computer. The former supplies a comprehensive conceptual framework—too comprehensive, in fact, to solve any specific psycholinguistic problems. As to the computer, its value is not just as a data processing device: it is an excellent partner to the experimenter and it can even take over the routine work with subjects while the experimenter does more interesting things, such as interpreting the data and asking new questions.

Some decisive psycholinguistic problems are currently being investigated in the 'project grammarama', so named because it employs a computerised device that displays strings of symbols whose grammar the subject is asked to uncover. In a typical case, the subject is given two keys to press: A and B, and his task is to find the grammatical combinations in an artificial language whose basic units are A and B. The machine will tell him whether his various special hypotheses are right or wrong. He will usually proceed by trial and error until he hits on the correct general formula, say AB^nA (i.e. an A followed by n B's and ending in an A). The subject should of course test this conjecture by trying to refute it, but what he usually does is to supply further positive evidence for his finding: the methodologically untrained college student is a born inductivist rather than a Popperian: 'Most people prefer to accentuate the positive.' The new machine makes fully automated psycholinguistic experiments possible. This automation not only saves much time but it eliminates many of the psychological resistances of the subjects, who do not mind being told by a machine that they are wrong.

Miller claims that the results obtained so far in 'project grammarama refute the principle of reinforcement—at least at this level, which is the one of cognition, not of mere automatic conditioning. The reviewer would have liked to see a caution to the effect that the failure of the current reinforcement theories at the level of concept formation does not invalidate them in other areas, much as the failure of thermodynamics at the atomic level does not refute it at all levels. In any case, if one is interested in explaining the cognition processes, then there is no escape from thinking in terms of the thoughts, choices, and decisions behind 'verbal behaviour'. Traditional behaviourism is then quite inadequate. But the objective approach of behaviourism, its insistence on clear thinking and exacting experimentation, have of course been retained: the defeat of behaviourism does not entail the triumph of wholistic psychology or of philosophical psychology. It is simply that a higher level in scientific psychology has been attained, one in which some of the traditional problems—eschewed by behaviourism—can now be clearly posed and even solved.

Altogether this is the shortest, most authoritative and most pleasant introduction to scientific psycholinguistics.

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David Knight: *Atoms and Elements* (London, Hutchinson, 1967, 30*s*.). Pp. 167.

This admirable short account of the vicissitudes of atomism in the Nineteenth Century is distinguished particularly by the philosophical literacy of the author. Dr Knight clearly understands the differences between positivists, fictionalists and realists, and illuminates his interesting subject by his grasp of what the various people were about. The chemistry of the nineteenth century emerges as a battleground between naive atomists, positivists and sophisticated atomists, the first exemplified by Dalton, the next by Wollaston in his later life, by Brodie and others, and the last by Davy and Faraday. This book disposes, once and for all, of the old myth of Dalton as the father of modern chemistry. Dalton's true significance, as Knight very clearly shows, lay in his exasperating greater men to an effort to clarify their own philosophical positions with respect to the logical status of theoretical concepts and so with respect to the kind of theory which should be striven for in chemistry.

Like the sixteenth century controversies over the reality of the motions of the planets, which brought out an identical collection of philosophical positions, chemical atomism in the nineteenth century illustrates, with particular clarity, the danger for science when healthy scepticism turns into philosophical positivism. On each occasion when positivists attempted to eliminate explanation in terrs of hypothetical entities in favour of deductively organised systems of generalisations of observed facts, they came to grief. Each time they tried to restrict explanation to deduction from covering law, the facts outpaced them.

And each time, the hypothesis of chemical atoms, electrically charged, and arranged in space to form molecules, was capable both of accommodating the facts previously known and accounting for those discovered.

On the historical side this book brings out very strongly the dominating position of Davy, in what actually took place in the nineteenth century. The threads of research and theorising which led to the ultimate triumph of Lewis in the twentieth century begin with Boscovich, and *Naturphilosophie*, are gathered up by Davy into a disciplined research programme, and passed on through Faraday and many others. The forces between point-atoms are electrical, Davy believed. Matter is not constituted of qualitatively different corpuscles, but of what we should now call fields of force. Davy's belief that the geometry of forces is the ultimate science, and that all matter is finally similarly constituted, is the view that has prevailed. The discipline of Wollaston's determinations of equivalents, the power and elegance of Brodie's calculus, the cool scepticism of Odling, the intransigence of Ostwald, look, in Knight's presentation, as the sideline grumblings of minor figures devoid of vision.

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354 *Brit. J. Phil. Sci.* 18 (1967), 354–356 *Printed in Great Britain*

Abstracts

Philosophy of Science, 33 (1966), No. 4.

Arthur H. Copeland, Sr., 'Mathematical Proof and Experimental Proof'.

In studies of scientific methodology, surprisingly little attention has been given to tests of hypotheses. Such testing constitutes a methodology common to various scientific disciplines and is an essential factor in the development of science since it determines which theories are retained. The classical theory of tests is a major accomplishment but requires modification in order to produce a theory that accounts for the success of science. The revised theory is an analysis of the nondeductive aspect of scientific reasoning. It results in a new definition of probability and a nonclassical point of view with regard to the foundations of probability.

Leon Cohen, 'Can quantum mechanics be formulated as a Classical Probability Theory?'

It is shown that quantum mechanics cannot be formulated as a stochastic theory involving a probability distribution function of position and momentum. This is done by showing that the most general distribution function which yields the proper quantum mechanical marginal distributions cannot consistently be used to predict the expectations of observables if phase space integration is used. Implications relating to the possibility of establishing a 'hidden' variable theory of quantum mechanics are discussed.

Herbert A. Simon, and Nicholas Rescher, 'Cause and Counterfactual'

It is shown how a causal ordering can be defined in a *complete structure*, and how it is equivalent to identifying the mechanisms of a system. Several techniques are shown that may be useful in actually accomplishing such identification. Finally, it is shown how this explication of causal ordering can be used to analyse causal counterfactual conditionals. First the counterfactual proposition at issue is articulated through the device of a belief-contravening supposition. Then the causal ordering is used to provide modal categories for the factual propositions, and the logical contradiction in the system is resolved by ordering the factual propositions according to these causal categories.

C. W. Rietdijk, 'A rigorous proof of Determinism derived from the Special Theory of Relativity'

A proof is given that there does not exist an event, that is not already in the past for some possible distant observer at the (our) moment that the latter is 'now' for us. Such event is as 'legally' past for that distant observer as is the moment five minutes ago on the sun for us (irrespective of the circumstance that the light of the sun cannot reach us in a period of five minutes). Only an extreme positivism: 'that which cannot yet be observed does not yet exist', can possibly withstand the conclusion concerned. Therefore, there is determinism, also in micro-physics.

D. H. Mellor, 'Inexactness and Explanation'

The paper discusses the problems raised by the inexactness of experiential concepts for a deductivist account of theoretical explanation. The process of theoretical explanation is explicated in terms of the devising of exact forms of inexact concepts. Analysis of the adjustments of concepts and their exact forms to each other reveals an implicit criterion of adequacy for theories which is related to the principle of connectivity.

John Kekes, 'Physicalism, the Identity Theory, and the Doctrine of Emergence'

I. Physicalism$_1$ and the weak identity theory deny, while physicalism$_2$ and the radical identity theory assert, that raw feels can be accommodated in a purely physicalistic framework.

II. A way of interpreting the claim of physicalism$_1$ is that raw feels are emergents.
III. The doctrine of emergence asserts that: (i) there are different levels of existence, (ii) these levels of existence are distinguishable on the basis of the behaviour of entities of that level, and (iii) an adequate scientific explanation of a lower level is inadequate for a higher level.
IV. The criteria of emergence are novelty and *a priori* unpredictability.
V. Either qualities or laws or both may be regarded as emergents.
VI. If the alleged impossibility of explaining higher level phenomena on the basis of explanation adequate for lower level phenomena is logical, then the impossibility is either trivial or it implies indeterminism.

Consideration of theories of emergence:

VII. Semantic emergence. On this view emergence is theory-bound, analogous to indefinability or indemonstrability.
VIII. Methodological emergence. On this view what we regard as emergents depends on the theory we construct.
IX. Nomological emergence. On this view emergence is an empirical problem.

Conclusion: if there are emergents, physicalism$_2$ and the radical identity theory probably have to be abandoned. However, unless actual examples of emergents are produced both theories hold. The logical possibility of falsification is not an objection against physicalism$_2$ and the radical identity theory; it is one of their merits.

Philosophy of Science, 34 (1967), No. 1

D. H. Mellor, 'Imprecision and Explanation'

The paper analyses the role of measurable concepts in deductive explanation. It is shown that such concepts are, although imprecise in a defined sense, exact in that neutral candidates to them do not arise. An analysis is given of the way in which imprecision is related to generalisation, and it is shown how imprecise concepts are incorporated in testable deductive explanations.

Herbert Hochberg, 'Dispositional Properties'

An analysis of problematic dispositional predicates like 'soluble' is presented. The analysis attempts to combine cogent features of opposed previous analyses of Carnap and Bergmann, while avoiding problematic features of both. The suggestion that there is an ambiguity in negations of assertions of dispositional properties, and a consequent distinction between 'not soluble' and 'insoluble' lies at the core of the solution.

Alex C. Michalos, 'Postulates of Rational Preference'

The postulates of rational preference suggested by Von Neumann and Morgenstern have been defended as descriptive or empirical generalisations and as normative principles. It is argued that the postulates are inaccurate empirical generalisations and unacceptable normative principles.

Leon J. Goldstein, 'Theory in History'

Present-day interest in history among philosophers seems largely limited to a debate over the nature of historical explanation among those who for Humean reasons insist that all explanations must rest upon general laws and history cannot be an exception to this, and those who say the historians do explain and since they do not use general laws the Humean claim is obviously mistaken. Like the latter, the present paper takes the explanations of historians seriously, but unlike the latter it is not willing to limit the role of philosophy of history merely to the elucidation of the language in which historians give final expression of their work. Rather it recognises that those explanations themselves set the stage for further inquiry in that *they* are required to be justified. It is here that the theories or general laws demanded by Humean analysis come in. After examining examples of general laws offered by way of example of what the 'Humean' philosophers are supposed to have in mind, each of which is an immediate generalisation of the explanation it is called upon to explain and is, hence, an instance of what has been called 'the dogma of universality', some examples of more promising prospects of theory in history are examined.

W. A. Suchting, 'Deductive Explanation and Prediction Revisited'

The paper has two main aims. The first is to reformulate Hempel's version of the thesis of the symmetry of explanation and prediction, as regards the deductive covering-law model, so as to generalise it and make it no longer subject to some of the criticisms which have been directed at it (Section II). The second aim is to consider, with special critical reference to Hempel's recent treatment in *Aspects of Scientific Explanation* (New York and London, 1965), some central criticisms of both the constituent parts of the above symmetry thesis, viz. that adequate explanations are potentially predictive (Section III) and that adequate predictive arguments are potentially explanatory (Section IV).

Dialectica, **20** (1966), Nos. 3/4

P.-E. Pilet, 'Avant-propos'
M. Bernard, 'Orientation et sélection pour la recherche dans les sciences de la Nature'
P.-E. Pilet, 'Vers un enseignement nouveau de la biologie'
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D. Reymond, 'Biologie et recherches pratiques'
P.-E. Pilet, 'Questionnaire sur l'enseignement des sciences biologiques'

Le fascicule 3/4 du volume 20 (1966) est presque entièrement consacré à la Biologie. Discipline presque exclusivement descriptive au début de notre siècle, cette science est devenue de plus en plus explicative.

Un enseignement, qui ne serait pas fondamentalement repensé ne pourrait que déformer les hypothèses, les théories et les faits actuels et donnerait des sciences biologiques de notre temps une image fausse.

Un des premiers problèmes qui se pose est évidemment celui de savoir quels sont les critères essentiels relativement à l'orientation et à la sélection des jeunes gens qui se destinent à la carrière de biologiste. Madame M. Bernard, attachée de recherches à l'Université d'Alger, nous fait part—dans le premier article de ce numéro de *Dialectica*—des résultats d'une vaste enquête qu'elle a menée pendant plusieurs années. Les questions abordées ne concernent d'ailleurs pas que les futurs biologistes mais d'une façon générale tous ceux qui sont attirés par les 'sciences de la Nature'.

Dans le second article, le Professeur P.-E. Pilet, directeur de l'Institut de Biologie et de Physiologie végétales de l'Université de Lausanne, s'attache à caractériser l'orientation nouvelle de la biologie et tente d'en déduire quelques idées directrices pour un enseignement mieux adapté et plus conforme aux sciences de la vie telles qu'elles se présentent maintenant.

Puis le Professeur H. E. Street, de l'Université de Swansea expose les efforts et les réalisations poursuivis en Angleterre dans le domaine de l'enseignement de la biologie.

L'importance des sciences dites fondamentales, dans la formation du biologiste est évidente. Encore faut-il—dans la perspective d'un enseignement restructuré des sciences biologiques—leur donner la just place qui leur revient. C'est de ces 'sciences supports' qu'il sera question dans le rapport rédigé par le Professeur R. Heller, qui enseigne la physiologie végétale à la Sorbonne.

M. L. Margot, maître au gymnase de Lausanne souligne, dans son article, les problèmes que posent les enseignements théoriques et pratiques de biologie dans les classes secondaires.

M. D. Reymond discute, dans son rapport, des problèmes pratiques qui attendent les biologistes engagés dans l'industrie.

Pour terminer ce fascicule, et répétant une excellente expérience faite en 1965 pour les mathématiques par le Professeur A. Wittenberg, le Prof. P.-E. Pilet a rédigé un questionnaire sur l'enseignement de la biologie.

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